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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Inaugural Address to the Royal Medical Society of Edinburgh at the Opening of the 137th Session.* By Professor MACLAGAN.

WHEN your President some time ago asked me to deliver an inaugural address to the Royal Medical Society, I found myself in the unhappy position of halting between two opinions. On the one hand, there was the desire to do any little service in my power to a Society from which I had derived many lasting benefits, and which in my early life had done me the honour to make me one of its Annual Presidents. On the other hand, there was at the time the doubt in my mind whether I would be physically capable of discharging any such duty in a manner satisfactory to you and to myself. I knew that I would have to address an audience capable of being critical, which I believed to be incapable of being exacting, and which I felt sure would make allowance for the adverse circumstances, personal and professional, which have prevented me from devoting to this interesting duty as much time and energy as I could wish. I accordingly, though with many misgivings, resolved to appear before you to-night. When, however, I had made up my mind to give an affirmative answer to the request of your President, there rose up before me the difficulty as to the theme on which I should address you. I knew that there were some topics of professional interest, on which I had meditated a little, which might profitably engage our attention for a few minutes; but I felt that to discourse to you upon a subject of practical medicine or sanitary science was merely to change the scene, and to be discussing here matters which, to use the parliamentary phrase, are properly dealt with in "another place," and that to many of you whom I had had the privilege of addressing as pupils, it might be only a weariness which should extort from you a groan of *Ohe! jam satis.* In these circumstances it appeared to me that as I was to find an audience, so I might find a subject in the Society itself, and I purpose therefore briefly to say something to you as to the Royal Medical Society.

The history of the origin of this Society, now entering upon its 137th session, is sufficiently well known to render much detail upon this point unnecessary. Though often told, however, the story will bear repetition, and it may possibly be that there are some strangers present to whom it may be new. I prefer giving it in the words of Dr Lettsom in a note to his Life of Dr Fothergill:—

> "Nearly fifty years ago several students of medicine formed themselves into a Society for their mutual instruction and advancement in their studies. Every student of a certain standing who distinguished himself by his diligence, capacity, and conduct, was initiated into this little assembly. Here the opinions of the ancients, of their contemporaries, nay, the doctrines of their masters, were frequently discussed; and two of the members were always charged with the task of providing instruction and entertainment for the next meeting of the Society. Questions no doubt were here disputed and decided which long experience would have declined; but it exercised their faculties, gave them both sides of arguments, taught them to doubt, and habituated them to observation. When one considers the activity as well as high reputation of a Society begun and conducted by students, curiosity is naturally excited to know the commencement of this successful and useful institution, which I am enabled to do by the information of one of its first and most respectable members. In the latter end of August 1734, the underwritten gentlemen (Dr Cleghorn, Dr Cuming, Dr Russell, Dr Hamilton, Mr Archibald Taylor, Dr James Kennedy), then fellow-students in the Schools of Medicine at Edinburgh, who had long been familiarly acquainted, and entertained a reciprocal regard for each other, after having employed themselves during the three preceding weeks in the dissection of a body in the anatomical theatre, agreed to spend a social evening together at a tavern. After supper it was proposed by one of the company that this little Society should meet once a fortnight early in the evening at their respective lodgings; that a dissertation in English or Latin on some medical subject should be composed and read at each of these meetings, to which such objections as occurred to the rest of the company should be made, which the author was to obviate in the best manner he could. This proposal was cordially assented to by all present, and Dr Cuming was appointed by the other members to prepare a dissertation for their first meeting on the signs, causes, and methods of cure of the *Rabies Canina*. This he accordingly did, and read it to the Society on the 20th December following. Dr Russell followed in one, *de Gonorrhœa Virulenta*. Then came Dr Cleghorn, *de Epilepsia;* Kennedy, *de Fluxa Mensium*." Etc., etc.

In the meantime I content myself with merely naming these original members, and may have a few words to say regarding them individually by and by. This was in reality the first attempt to found the Medical Society, for it had not yet become Royal; but in truth it did not at once succeed, for in the summer of 1735 these friends were dispersed, Cleghorn alone remaining with his friend Fothergill to attempt to carry it on. It does not appear that much, if anything, was done during 1736, but the seed had been sown though it was long of germinating, and it was not till 1737 that it fairly sent up the stem and down the roots which have grown into the goodly tree under whose shade we are met. Such is no unusual history of similar institutions. It is with them as with plants. The herbaceous annual springs up aye, and flowers readily, only, however, to perish under the first frost; but the hard cone and sturdy acorn lie longer in the ground, and when they do sprout it is to meet the vicissitudes of the weather with firm defiance. Such has been the Royal Medical.

Ours is no sapling, chance-sown by the fountain,
Blooming at Beltane, in Winter to fade.
When the whirlwind has stripp'd every leaf on the mountain,
The more shall Clan-Alpine exult in her shade.

Let us, however, turn from the early struggles of the Society, to the circumstances under which the founders met, and the objects which they had in view. It seems that their first meeting was in a tavern for social purposes. They turned this fortuitous assembly into an association for mental improvement, and there seems to be little doubt that in addition to this they had in view the desire to help each other in their preparation for taking their Degrees. Let us look for a moment at each of these objects of their meeting, and see whether or not we may derive some instruction from them. First, they met for social purposes. You may smile perhaps when you read of the foundation of a scientific society being laid in a tavern. I do not quote this incident as indicating the proper locality for the foundation of a learned body, nor do I cite to you the doctrine of that bard of Bacchus, Walter de Mapes, the Anacreon of the twelfth century, who is said to be—though, to his credit as an archdeacon, it is doubtful if he is—the author of the famous song beginning—

"Mihi est propositum in taberna mori,
Vinum sit appositum morientis ori,
Ut dicant cum venerint, angelorum chori,
Deus sit propitius huic potatori."

No; you must bear in mind the custom of the times. There was no comfortable hall for them to meet in, and the lodgings of a student in the early part of the eighteenth century—probably a lofty tenement in the High Street or Cowgate—were not likely to afford much accommodation for a knot of students intent on a little conviviality. Remember that long after this lawyers met their clients, merchants their customers, and doctors their consultants, in a tavern. I beg you, however, to observe under what circumstances they met socially. It was after they had been three weeks engaged in dissecting a body at the anatomical theatre. Here is a good example—work first, and pleasure afterwards. Write that in letters of gold on your scheme of life. These men met, however, for social purposes, and I have no idea that they sat dry-lipped; but it is clear from the result that theirs was more the 'feast of reason and the flow of soul,' than the mere ministering to sensual indulgence. Let me not, however, be held as throwing the wet blanket of censure on social meetings. I know the benefit which they have conferred upon the medical profession, and I am the last man to say them nay; but there is a time for all things, and I say emphatically that mere convivialities for those who are in their student's period are upon the whole to be avoided. Do not take umbrage at this, and resent it as an expression of opinion that young men cannot meet convivially without running into excess. I hope that all of you are willing and able to do this; but in spite of your best resolutions there is a risk. In giving this advice, I no more say that young men will under such circumstances break away from all proper restraint, than I say that the young colt will necessarily be a bolter because he kicks up his heels after a full measure of corn; but I would rather keep his blood cool in his hot youth, and would reserve an extra feed for him when he is thoroughly well broken and has become a steady going roadster. And thus I would have you look forward to the enjoyment of social intercourse with your mates as a reward for your labours when you are fairly in harness and are doing your good day's work. I do not take any exception, however, to your yearly festival, where, in the company of your seniors (who by the by always greatly enjoy it), you recall the ancient glories and speak to the present prosperity of the Royal Medical.

Let me say further, that independently of all conviviality, there was in the meeting of these original members an object in view which I cannot too highly commend to you. It was the formation and cementing of personal friendships. There is to my mind nothing more charming than those personal friendships which we make in early youth, which are independent of all relationship, and are based only on a community of feeling and interest in a common pursuit. A true personal friendship of this kind is of inestimable value; and from the very fact of its neither being, as it were, forced upon us by affinity, nor dictated by self-interest, is of the greatest and purest value. "Amicitia," says Cicero, "res plurimas continet. Quoque te verteris præsto est; nullo loco excluditur; nunquam intempestiva, nunquam molesta est. Itaque non aqua—non igni, ut aiunt, pluribus locis utimur quam amicitia." And, Gentlemen, please remember what is the real meaning and value of a friend. It is not merely to have a person who will consort with you in hours of leisure, and be to you a source of personal enjoyment. The real friend is he who, taking an independent view of our conduct and opinion, will frankly criticise them, and freely remonstrate if he sees occasion. If you form a friendship within these walls—and you must be a singularly isolated being if you do not—bear in mind that this is your responsibility. Watch over your friend with the jealous eye of a brother, and keep him right in every respect if you see in him any tendency to go wrong. I speak not of moral conduct only, but as regards his studies and his work. Aid him if you can in carrying out his studies; state to him your own difficulties in scientific matters; explain to him if you can, or, if not, try to work out with him the difficulties which he has met with. Above all things, keep him in the earnest, steady following of the path of honourable Christian duty, and be encouraged by the words of the apostle—"For in doing this thou shalt both save thyself and them that hear thee."

The second object that these men had in view was their mutual improvement, and this is the real aim of any such Society as ours. It is clear that they felt, what every true student of our profession must have experienced, that the mere attendance on prelections is not the study of medicine. Something more is required than that a man's brain should be a sponge to absorb as much as he can hold of what is instilled into him by others. This is to be taught, not to learn. I am sure that I speak the sentiments of every other Professor (they are assuredly my own), that the man who merely, without independent thought, accepts our teaching, and holds that to be his store of learning, does not do justice either to himself or to us his teachers. Listening to lectures and reading books, though essential, ar· not all that make good study. The Latin word "studium" has a signification that is not easily defined. It certainly means study in the ordinary acceptation of the word, but it means much more—earnest application, zeal if you will, and it certainly implies the idea of independent thought. Now, this is what these men aimed at. They were attending the prelections of the most eminent teachers then in existence, if we except those of Leyden, but they were not content with hearing them. They wanted to think over them and judge of them; and they saw that there was no way in which they could do this better than in mutually discussing what their teachers had laid before them. And this is the real benefit of such a Society as ours. The leading facts of a science may by lectures and books be put into our heads, but the true comprehension of it is gained only by thinking it out for ourselves, especially in company with others, and by seeing all its bearings on collateral subjects when these are brought before us by a free discussion.

It would appear further, that, in addition to the mere acquisition of knowledge, our founders had in view the subsidiary object of aiding one another in preparing to take their Degrees. It is probable that this in great measure referred to the defence of the Thesis, which was at that time one of the most, if not the most, important part of the trial of a candidate; and we can readily imagine how the practice of discussion was found by them to be an excellent preparation for this. Even, however, in our modern days of oral examination, a discussion at the Royal Medical may be a direct help to a candidate. I can speak from experience. One of the first questions put to me in botany by Professor Graham, when I went up for my degree, referred to a subject of vegetable physiology, which had escaped from my mind, if it ever had been there, but which had been brought out in a discussion in the Royal Medical the very night before I made my appearance in presence of the "most potent, grave, and reverend signiors" of the medical faculty. Such occurrences, however, are mere accidents, and are of comparatively trivial importance. I know that to all of you who have not yet taken your Bachelor's degree, your examinations are—they certainly ought to be—subjects of much thought. I have no belief in the man who professes to take his examinations lightly. If he has been a hard worker, he has the honourable ambition to get honours as well as a diploma. If his conscience tells him that he has not worked so profitably as he ought to have done, he has before him the anxious thought that he may get neither the honours nor the diploma. I assume, therefore, that all intending candidates are anxious as to their examinations. Do you ask from me, who am both a Professor and Examiner, what I think is your best way for preparing for this ordeal, I reply in general terms, your best preparation for examination is the preparation for the work of actual practical life. I anticipate the reply—"Oh, but much of what we have to pass is pure science, which is of little use in the exercise of our profession, for we know that we must be prepared on many subjects which may safely be forgotten when we go into practice." May they safely be forgotten? That is just what I deny. Undoubtedly no man can keep in his mind, or need keep in his mind, the details of the natural sciences which he has acquired as a student; but the broad principles of them remain, and how can these be known without something of the details? But are these principles which you wish us to retain of any use? I answer that I know not any branch of medicine which may not have light thrown upon it by pure science, and I know not in what quarter pure science may not thus become practical. Is antiseptic surgery a practical question or not? Yet its whole theory and practice are based upon such purely scientific matters as bacteria, Pasteur's liquid, and high magnifying powers. No doubt, if medicine is merely to be a matter of routine practice, pure science may be shut out; but if you displace Medicine from her scientific throne, what a fall will be there! This is a consummation most devoutly to be deprecated. Do not, therefore, I beseech you, grumble at the work you may have to do in studying subjects of pure science, and welcome above all things any discussions which may make you think on such matters more closely and earnestly.

The advantages then contemplated by our founders were the social benefits of friendly intercourse, the enlargement of their personal knowledge, and the preparation of each other for their academical trials; and these advantages the Society still offers. But are these advantages as fully appreciated as they ought to be? I answer, No. I think many of the students of this School do not know how precious an adjuvant to the teaching power of Edinburgh the Royal Medical Society is. No doubt you have a constant succession of fresh additions to the roll of ordinary members, but there ought to be more. Your new members may be taken upon an average at 25 to 30 yearly. There are at least 800 medical students in Edinburgh. I divide this number by 4, the number of years that they are supposed to study here, and this gives, taking the new entrances at 30, little more than 1 in 7 who join the Society. Surely the majority do not know the benefits to be derived from the membership of this, or, let me in fairness say, any Society with similar objects and as good an organization, for I am not here as a "touter" for new members, nor does the Royal Medical require it. I grant that membership costs some money—not much in all, and that diffused over several years; but if I had the ear of the parents of those coming to study here, I would say, In calculating the outlay of your son's education, put down £10 for his attending the Royal Medical, and I promise you that it will be well-spent money!

Some, I believe, hesitate, because they think they cannot spare the time of one night weekly during the session for the enforced attendance here. The idea is, I believe, founded on error. I think there is often too much continuous reading on the part of earnest students. Give to a man who really reads with his brains, and not with his eyes, Monday, Tuesday, Wednesday, and Thursday for his professional books, and I will guarantee him to be well up for his examinations although he gives Friday to the discussions here, Saturday to healthy relaxation, and Sunday to the study of those things which concern the higher life. No doubt, it may be a lost day if not properly used. To come at the due hour to answer the roll-call, to listen to the dissertation, and then go home, is to spend the night by no means profitably. It is only listening to a lecture, and there is no need to come out at night to do that. But the Friday will be another night altogether to a man who determines to put it to the right use. Let him see some time before what is the subject of the dissertation; let him read up for it carefully; let him work out from the library any collateral points which the author of the dissertation may have touched upon, or which appear to him to have been omitted, and he may depend upon it that he has spent his time well. But let him also resolve not to be a silent member; let him be determined to speak, let it be only a single sentence. He may be nervous about his first appearance, but maiden speeches, whether delivered here or in Parliament, always, in accordance with our British rules of fairness, meet with a generous reception. He will soon feel at his ease, and his next speech will be better than its predecessor. It is of more consequence to a man in life than many imagine, to acquire the habit of expressing one's self with ease and fluency. A man need have no idea in his mind of ever becoming a lecturer or public speaker; but in society or say in consultation with a professional senior, to be able to express himself with ease and in good English, marks the well-educated and accomplished gentleman.

Let me say a few words on the selection of subjects for dissertations. You may remember that in the sentence which I quoted from Dr Lettsom, or rather from Dr Fothergill, for the words are his own, he says that the founders discussed subjects which long experience would have declined. Avoid this error. Choose rather those subjects which involve the theory than the practice of medicine. It is no disparagement to you to say that you have not the experience which will enable you profitably to discuss purely practical subjects; and those that involve theory are more open to diversity of opinion and more prolific of good discussion. Practical subjects need not be tabooed. A good case from the Hospital, reported by him who has perhaps had it under his observation as a resident or a clinical clerk, forms a most legitimate subject for a communication; but for a formal dissertation the best subject is one which involves scientific or theoretical considerations. Of course it may be said, that to do justice to such subjects, experimental investigation is called for, and the student has no time for this. No doubt this may be true, but a first-class dissertation may be produced which shall do no more than point the lacunæ which ought to be filled up by research; and the member, who has thus made himself thoroughly acquainted with the actual state of science regarding his subject, may afterwards turn the work to profit by enlarging his dissertation by original research into the form of a good thesis when he comes to take his Doctor's Degree. The real duty of a member, therefore, is to make up his mind to take part in the public business of the Society; but let me at the same time say, that the private business ought not to be despised. I have no respect for those men who come in for the hour of private business, and leave the hall before the debate; but during the busy time of medical study you have no opportuntity of acquiring what are commonly called business habits, and these are most important to every man in after-life. Nowhere will you gain this better than where you take part in business which is open to the criticism of others. Cultivate business-like methodicity in your own affairs. It saves time, which is money; it leaves you the clear and unconfused use of your brains, which is better than money; but it is a poor view of methodical habits to think that these are useful only to help you in practice. A medical man, though he becomes a busy practitioner, does not cease to be a citizen; and unless he makes up his mind to become a mere scraper together of fees, he will be inclined, yea, feel himself bound to take part in matters which are for the good of the community. He will find that business habits acquired in the Royal Medical may stand him in good stead when he comes to take part in the work of public or benevolent institutions, with which, either voluntarily or officially, he may become connected. Look, for instance, at the much-sought-after appointments of Medical Officer of Health, and see how much of their duties require for their due discharge regular business habits.

I have been hitherto speaking of the objects which the original founders had in view, and I daresay you will all think that I have wandered away from my text; but it was having this leading idea in my mind that has kept me hitherto from alluding to one of the chief advantages of the Society—its library. But this was not started till long after the day of the founders, and I rejoice that the Society has always had funds, and has had the good sense to spend them, in maintaining this noble collection of books. There is one point, however, in regard to the library, to which I wish specially to direct attention—I mean the importance of keeping up *au courant du jour* the *Catalogue Raisonné*. You are aware that this was for the first time accomplished in 1836. It is not for me to say whether there was success or not in a work in which I had at once the honour, privilege, and advantage to take part; but I am sure of this, that this Catalogue has since then often been of great use to me, and I would like to see it brought up to the present state of science for the benefit of myself and others. I am led to understand that all the separate books added to the library since the above date have been duly classified, but nothing has been done in the way of adding to the Catalogue Raisonné, as suggested in the preface to the edition of 1836, the various important papers scattered through British and Foreign Periodical Medical Literature. Such an undertaking would require much labour and much judicious discrimination; but it is worth attempting. Let me offer a practical suggestion. Would it not be well not only to appoint a committee to see that all the books which have been added to the library, and the more important papers published in periodicals, have been classified during these thirty-eight years, but to have a standing committee who, meeting once or twice a year, should see every fresh book and memoir put into its proper scientific place in such a catalogue? I do not ask the Society to undertake the expense of a fresh print, but a complete interlined Catalogue Raisonné in the Library would be of inestimable value, and would of itself be a precious privilege to members.

I have thus, Gentlemen, made some observations on the objects which the founders of the Society had in view, and one of the ways, the library, in which these were carried out by their successors. Perhaps it may be of some interest to many of you to know something more of these founders than their mere names, and I venture therefore to give you a short history of one or two of them regarding whom we possess information. I have already told you, that although they were never members of the Society as formally constituted, there were in 1734 six gentlemen to whom we in reality owe the idea of its establishment. I take them in the order in which they are named by Lettsom, in the note formerly quoted from his life of Fothergill.

CLEGHORN.—George Cleghorn was the youngest of five children of a farmer at Granton, in this neighbourhood, and was born on 18th December 1716. He got the rudiments of education at the parish school of Cramond, and went subsequently to the High School of Edinburgh, where he distinguished himself by his acquirements in Latin, Greek, French, and Mathematics. In 1731 he began the study of medicine. He thus was only eighteen years old, and in the fourth year of his studies, when he took part in the attempt to found a Medical Society. Two circumstances seem to have been of great value to Cleghorn during his period of study. One was his having been placed not only under the tuition, but in the house of the first Monro, then building up that reputation which forms one of the glories of the Edinburgh School of Medicine. The other was his forming in 1733 the acquaintance of a young Englishman, John Fothergill, who became his bosom friend and inseparable companion. The habits of these two friends are thus described by Hutchinson, in his Biographia Medica:—"These pupils then studied together the different branches of science under the same masters with equal ardour and success. They frequently met to compare the notes which they collected from the professors, and to communicate their respective observations. Their moments of relaxation, if that time can be called relaxation which is devoted to social studies, were spent in a select society of fellow-students, of which Fothergill, Russell, and Cuming were associates—a Society since incorporated under the name of the Royal Medical Society of Edinburgh." Here, Gentlemen, we have an interesting specimen of that form of friendship which I have already commended to your consideration, doubly interesting to us, because to this intercommuning between Cleghorn and Fothergill we must, I think, trace the origin of those meetings which expanded into the Royal Medical Society.

Cleghorn's subsequent career was one of distinction. In 1736, when yet only twenty years old, he was thought so highly of, that he was, on the recommendation of Dr St Clair, conjunct Professor of the Theory and Practice of Medicine, appointed surgeon to the 22d regiment of foot, which was then stationed in Minorca. In this limited sphere he remained thirteen years, but he was not the man to let time hang heavy on his hands. He dissected human bodies, got monkeys sent from Barbary that he might work at comparative anatomy also; and besides doing his military duties diligently, studied the diseases of the island. All this while he kept up a constant correspondence with his dear friend Fothergill, who was indefatigable in procuring books for him in London. The results of Cleghorn's observations were given to the world in his work on the Diseases of Minorca, which he published on his return to Britain. He went with his regiment to Ireland in 1749, passed over to London to supervise the publication of his book in 1750, and there renewed his anatomical studies by attending the lectures of Dr William Hunter, the celebrated elder brother of the still more celebrated John Hunter. This fresh contact with anatomical teaching seems to have determined the future course of Cleghorn's life. He left the army in 1751, went to Dublin, and began to give lectures on anatomy. He was successively lecturer on and professor of Anatomy in the Dublin University, and was one of the original members of the Royal Irish Academy. Cleghorn lived and died a bachelor, but in default of family of his own he tenderly fostered the children of a deceased brother, one of whom he educated to the medical profession, but who died at an early age in 1784. Cleghorn secured for himself an independence, and became the possessor of considerable estates in Ireland, in his later years spending his leisure in farming and horticulture. He died in 1789.

FOTHERGILL.—Of Fothergill I must say a few words, although, to use a legal phrase, I may be thought to be going beyond the record, but I hope to show that I am really within its limits. In the list given by Dr Lettsom, in his note to the life of Fothergill, formerly quoted, he names the six who in 1734 met to discuss medical matters together; but among these Fothergill does not occur. Lettsom, however, makes this remark—"Why Dr Fothergill was not associated into this little band cannot now be recollected, but he was known and highly esteemed by every one of the members." Fothergill, let us bear in mind, was a Quaker. The first meeting, which is what is here recorded, was in a tavern, and perhaps his scruples may not have permitted him to join in this reunion, however little it may have partaken of the character of a "jollification." It does appear, however, that Fothergill joined in some of these subsequent meetings, probably in the early part of 1736, when all the others of the original sederunt had left Edinburgh, except his beloved Cleghorn, who was soon to depart also. This seems clear from a note appended to Fothergill's "Life of Dr Russell," in which, speaking of a Thesis dedicated "Societati Medicinæ Studiosorum in Academia Edinburgena dudum institutæ," he quotes from another Thesis the following words: "Floruit triginta prope annos hic juvenilis circulus et ut æternum floreat præcor;" and to this he adds: "Soon after its first institution the writer of this memorial"—*i.e.*, the Life of Russell—"was likewise a member, and knows from experience the benefits resulting from it." Fothergill therefore, though his name does not appear in the list given in his life by Lettsom, or in any of the subsequent printed lists, had actually been one of the little band who met previous to the formal institution of the Society in 1737; and, moreover, he had held this to be the Royal Medical when he speaks, writing in 1769, of his having been a member of the Society. It may be supposed that the note from which I have quoted is not Fothergill's own, but Lettsom's, the editor of his life and works, but this cannot be, for John Coakley Lettsom does not appear in your lists till 1787-88, which cannnot be said to be soon after its first institution; and, moreover, it does not appear that Lettsom ever was an ordinary member, but was an honorary member, having been elected at this time, that is, four years after the publicatisn of his "Life of Fothergill," a work which probably gained for him this merited honour. Were Fothergill now alive he would, I think, maintain that our Society dates not from 1737, but from 1734.

Of Fothergill's writings I do not purpose to speak. I cannot spare time even for a bare enumeration of his many interesting papers. I merely note that, in addition to his numerous practical writings, he published many interesting botanical memoirs—a circumstance noteworthy in the case of a physician whose practice had attained the value, enormous in those days, of £7000 a year. Fothergill died on 26th December 1780.

RUSSELL.—The next of the six of 1734, whom I would like to notice shortly, is Dr Russell. Of him we have a full account by Fothergill, who wrote a biography of his friend. Alexander Russell was the son of a successful Edinburgh lawyer, and, like Cleghorn, was a pupil in that admirable High School, which, as a six years' *alumnus* thereof, I cannot mention without pride and satisfaction. He was apprenticed to his uncle, who was at that time a leading practitioner in Edinburgh. He studied medicine here during the years 1732–34, under Monro, Rutherford, Alston, and the other then celebrities of the Edinburgh School. It is remarkable that Russell, though so true a student and afterwards so distinguished a physician, left Edinburgh without taking his degree. We know nothing of why he did not do so. There is no evidence that he tried and was plucked. If he did try and failed, let his instance be a consolation—I do not say an example—to those who may be unsuccessful. It is more likely that he found an opening to take him abroad before he could go through the ceremony of graduation, for there was no law in those days to score him down as an unqualified practitioner. Be this as it may, he visited London in 1735, and from it he went to Turkey, settling in Aleppo in practice in 1740. He soon found that the practitioners of that place were an ignorant race. He had knowledge to communicate to them, and he did so. Mark that the real generous-minded man of science does not keep his knowledge to himself, and in so doing Russell acquired such a reputation that not only the strangers in Aleppo, but the followers of the Prophet flocked for advice to the Scotch Christian doctor. He gained the confidence of the Pacha, and acquired an ascendency over him even in matters pertaining to the administration of justice. In Turkey Russell came into contact with the Plague, and so carefully observed that malignant fever that he became the leading authority in regard to it, and was so highly esteemed in this capacity, that when the disease appeared in Lisbon, and it was feared that it might invade Britain, he was consulted by the Privy Council as to the best means of averting it from these shores. But Russell did not confine his work to merely practical matters. He also was a scientific enthusiast, and to him we owe some interesting botanical information, especially as regards the plant which yields scammony. It was in his "Natural History of Aleppo," published in 1755, a work which was translated into various European languages, that he gave to the world his account of the Plague and of his various observations in natural history. He settled in London in 1759, was chosen a physician of St Thomas's Hospital, was elected a F.R.S., was also a member of the Medical Society of London, and died in the metropolis in 1770.

CUMING.—William Cuming is an example of the successful provincial physician of a century ago. He was the son of a merchant of Edinburgh—that is, probably, of one of the shopocracy of the Scotch metropolis—and was born in 1714. He studied medicine not only in Edinburgh, but in Paris and Leyden, and was ultimately, through the influence of Fothergill, led to settle in Dorchester. In this county town he spent his whole professional life, although his reputation was so great that he was urged to come to London to succeed Russell. He seems to have acted wisely, for he became not only the trusted medical adviser, but the personal friend of all the leading families in the county of Dorset. In Cuming, as in Russell, we have an instance of a physician practising for some time without any degree, for Cuming graduated at Rheims in 1750, and also at Edinburgh in 1752. This last degree was probably taken to enable him to join the Royal College of Physicians of Edinburgh, of which he was made a Fellow a month after his graduation. Dr Cuming seems to have been no contributor to medical science, for I do not find any record of publications bearing his name. He died in the 74th year of his age, in 1788.

Of the remaining three of the original members of the meetings of 1734, I have nothing to say but what is recorded by Fothergill.

HAMILTON.—Alexander Hamilton was the son of the Rev. Dr William Hamilton, who was elected Professor of Divinity in Edinburgh in 1709. Alexander took his degree at Edinburgh in 1737, his thesis being De Morbis Ossium. He emigrated to Annapolis, in North Carolina, and died there.

TAYLOR.—Of Mr Archibald Taylor, we learn only that he was brother to a physician of that name in Edinburgh, that he was an "ingenious" young man, and that he died a few years after in the East Indies.

KENNEDY.—Of the last of the six, Dr James Kennedy, we learn that he took his degree in Edinburgh, that he went to travel with a young gentleman of fortune, that he remained three years in this employment, but that, reversing the maxim, "*Cedant arma togæ,*" he gave up the doctor's gown for the military jacket, and the lancet for the sword, and the last thing that is heard of him is, that he had become a senior captain of dragoons. Whether Captain James Kennedy, M.D., died the hero-death on the field of fight, or subsided into a half-pay fogie, with interminable stories as to "our army in Flanders," I have not been able to expiscate.

I feel that in strict propriety I ought not to have said so much as to these men of 1734, but ought rather to have endeavoured to fix your attention on the men of 1737, who were the original members of the Society when formally constituted. They were eight in number, for in this matter I adhere to the old style of reckoning dates, which was not changed till 1752. There names are, George Armstrong, James Scott, Michael Dickson, Archibald Drummond, Robert Gusthart, Thomas Wilkinson, Robert Pringle, Stuart Threipland. With regard to four of these—Armstrong, Scott, Wilkinson, and Pringle—I can say nothing; but I wish you distinctly to understand that this arises from my having been unable go into the work of searching them out diligently. Three of the others graduated together at Edinburgh in 1740, Michael Dickson's thesis being De Purgantibus; Archibald Drummond's, De Urina; and Robert Gusthart's, De Aere. The last of the eight, Stuart Threipland, graduated in Edinburgh in 1742, his thesis being De Partu. He was admitted to the Fellowship of the Royal College of Physicians of Edinburgh on 7th February 1744, and was elected President of the College on 4th December 1766. With regard to this gentleman I am able, thanks to the courtesy of his grandson, the present Sir Patrick Murray Threipland of Fingask, to give you some precise information. In reply to my request, the honourable Baronet kindly sent me the following notice regarding his ancestor, and I am sure that you would not pardon me were I in any way to alter a narrative so interesting and so graphic:—

"My great-grandfather, Sir David Threipland, who was attainted in 1716, was twice married, his first lady being a daughter of Sir James Ramsay of Banff, and his second Miss Catherine Smythe, the heiress of Barnhill, near Perth, and who was the mother of Sir Stuart (Dr Stuart), to which property Sir Stuart succeeded on her death. Sir David by his first wife, to whom he was married in 1688, had seven sons and three daughters; and by his second, whom he married in 1707, he had two sons and four daughters. Sir Stuart was born on the 26th May 1716. Though the youngest of all the family and prematurely brought into the world, being a seven-months' child, so tiny and delicate as to require being placed in cotton, yet he lived to the great age of 89, all his brothers and sisters having predeceased him. The story of the accouchement of Lady Threipland and the baptism of the child is very peculiar. When the troops of the Government had possession of Fingask Castle in 1715, and some of the soldiers were even quartered in the house, Lady Threipland became alarmingly ill, and in the midst of much anxiety and care—her husband and sons at a distance, uncertain of their fate, and the cause, in which they had embarked their all, giving way on every side—her child, afterwards Sir Stuart, was born prematurely. It was thought that under all the depressing circumstances of her situation she could not survive, and a clergyman of the Episcopal Church in Perth was sent for privately, the clergy of that persuasion being marked men at that period as known adherents of the Jacobite cause; and having administered the sacrament, he proposed, as so favourable an opportunity might not soon occur, to baptize the child. This suggestion, communicated in a whisper to the nurse and others who were in attendance, was at once assented to by them, but the difficulty consisted in knowing by what name the infant should be called, his father having left no instructions, and his poor mother being thought much too weak to be consulted on the subject. The good lady, however, had heard a little of what was passing near her bed, and drawing back the curtain, called out in a faint voice, 'Stuart, Stuart.' This was enough, and by that name accordingly was my grandfather christened before the clergyman left the house. Sir Stuart having thus been reared and cradled in a hotbed of Jacobitism, and having actually seen his aged father, Sir David of the "15," take a paralytic fit when pulling on his jack-boots to go out again for 'the good cause,' it cannot be wondered at that, on Prince Charles Edward's arrival in Scotland, he became one of his most devoted followers throughout the whole of the unfortunate enterprise of 1745-46. After the battle of Culloden, and many months of wandering among the hills with the Prince and his other followers, Sir Stuart contrived to escape to Edinburgh, where he was taken in charge by Mr William Gordon, a bookseller, who soon after having occasion to travel to London, took Sir Stuart with him disguised as one of his apprentices. Arriving safely in London, he immediately left for Rouen, where he joined a party of similar exiles, and put up in the house of Mons. Decamps, Professor of the Academy of Design, from whom it would appear Sir Stuart took lessons in design with considerable success. Amongst his companions of exile were William Hamilton of Bangour, Andrew Lumisden, who subsequently became private secretary to Prince Charles, and who wrote a learned work on the antiquities of Rome, and his (that is, Lumisden's) brother-in-law Robert, afterwards Sir Robert Strange, Sir James Stewart of Coltness, etc., etc. After the lapse of some three or four years, Sir Stuart appears to have availed himself of the 'amnesty,' and returned to his native country. In 1753, he married Miss Janet Sinclair, of Southdon, in Caithness, who died within two years after their marriage. Sir Stuart married secondly, in 1761, another Caithness lady, Miss Janet Murray, of Henryland and Toftingall, by whom he had one daughter and four sons, the eldest of whom was my late father, and who succeeded to the estates of his mother, and consequently had to assume the name of Murray along with Threipland. Sir Stuart, during the early years after returning from abroad, practised as a physician, but it could hardly be said with the ordinary professional views, his income from other sources rendering him quite independent. It is understood, however, that his services were always at the command of the Jacobite party, and particularly, if any who had suffered in the Stuart cause were in need and sickness, he proved himself to such, a 'hedge of shelter and a store of succour,' to use the expression of an individual who, it would appear, had benefited by his generosity.

"Sir Stuart was stately in his general demeanour. Refined in his own feelings and manners, all refined and elegant things had for him an attraction. This temperament, joined to his benevolent disposition, led him to patronize to the extent of his means many persons engaged in the elegant arts. He was a great promoter of an effort which was made in Edinburgh in the early years of George the Third's reign to introduce the English style of speaking and writing. The establishment of a riding-school in Edinburgh, under the care of an imported foreigner named Angelo, was partly brought about by him; nor were the useful arts below his regard. That of stereotyping, which was invented by an Edinburgh goldsmith of Jacobite connections, named Wm. Ged, was indebted to Sir Stuart to a considerable extent; and among the curiosities preserved at Fingask Castle are several plates of the edition of Sallust, which is noted as the first work ever printed in that manner. Though, as has been remarked, Sir Stuart was refined and elegant in manners, still there was no effeminacy in his habits. It is remembered that one day a chambermaid using particular pains to smooth his bed, he told her to take less trouble about it: 'How would you have liked to sleep for months as I have done on heather, with stones for my pillow?' This dignified old man never dined without remembering his political principles. The first glass of wine was invariably drunk to the 'Land of Cakes, and the right *Steward* to deal them.' Living to an unusually advanced age, he lost to some extent the original clearness of his mind, but still the feelings of his early days hung about him. When the news of the Peace of Amiens was imparted to him by a servant, he was heard to murmur, 'There'll never be peace till Jamie come hame.' He survived that event a few years, dying tranquilly in 1805 at the age of 89."

I am sure, Gentlemen, that no amount of loyalty to that dynasty which has given us that honoured Lady who now fills the throne, can prevent us from paying our tribute of admiration to—nay, even feeling a decided touch of sympathy for—the chivalrous devotion of such adherents of a failing cause as Stuart Threipland. *Victrix causa Diis placuit, victa Catoni.*

I have given you here some very imperfect biographical notices, under the idea that as every man likes to know something of his forefathers, so you might like to know something of that ancestry in this Society of which you are now the representatives. I have not offered it to you as a satisfying meal, rather as a whet which might excite in you an appetite for more. I cannot but think that some member of the Society, with a taste and willingnesss for that rather troublesome grubbing among books and records which is essential to biographical research, might bring out some interesting narratives as to those who have been your predecessors. I do not speak so much of the great names—the Monros, Cullens, and Gregorys—who are in your list, and who are, as it were, the big nuggets, because all of them have been already handled by biographical artificers whose style and workmanship it would be difficult to rival; but I suggest that possibly, by a little sifting and washing, such a member might find some good gold dust of which he might form a valuable jewel to ornament the Society. If such a literary work were undertaken, I should like nothing better than to be privileged to give to the author that attention, which you have, with too great good-nature, accorded to the present address.

ARTICLE II.—*The Morisonian Lectures, delivered before the Royal College of Physicians of Edinburgh: Session* 1874. By J. BATTY TUKE, F.R.C.P.E., F.R.S.E.

LECTURE III. (*delivered 24th March* 1874.)

Influences inducing and producing Brain Disease.

HAVING in the last two lectures considered the normal condition of the brain elements, their relation to each other, and the later theories of their action and function, we will proceed to the next stage by studying the various somatic conditions which act on them so as to produce solution of continuity. These conditions may be classed as follows:—

I. *Idio-encephalic* disease, or disease affecting primarily the cerebral tissues.

II. *Evolutional* conditions of the body concurrently implicating the brain.

III. *Morbid* conditions of the body concurrently affecting the brain.

In a paper published some years ago,[1] I employed the term

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[1] Journal of Mental Science, vol. xvi. p. 196.

Idiophrenic to express the first class of conditions; but Dr Bucknill has improved upon it, by using the more comprehensive word Idio-encephalic, which includes not only diseases of the brain substance proper, but also those commencing in the membranes and bones. In the same paper the pathogenetic classification was more diffuse; but the one now before you contains no departure from its principle, merely a modification resulting from extended experience and the study of the opinions of others. Pardon me, if I state now that in this course of lectures other modifications will be made—indeed, in one or two instances actual change of written opinions will be expressed; but I trust that vacillation will not be laid to my charge when resiling from positions which have been found to be untenable. Every man lives through many phases of opinion and belief as work and experience bear upon him, and I have yet to learn that he is the less wise man who openly recants or modifies theories expressed in his scientific juvenescence.

Were we to take the first class alone, and to conjoin the second and third, we would have Van der Kolk's simple classification of insanity; for by "Idiopathic" he expresses primary changes in the nerve structure, and by "Sympathetic" all changes caused by abnormalities of the system conditioning the brain. I adhere to the first position of this great teacher, but consider that the second may be with propriety subdivided, as it takes no cognizance of the normal abnormalities (if I may be allowed the paradox) concurrent with development and decay, apart from the consequences of bodily disease.

I propose considering the Idio-encephalic conditions productive of insanity under four heads—1st, Traumatic; 2d, Adventitious products; 3d, Over-excitation of the brain; and, 4th, Defective organization. The evolutional conditions, under five heads: 1st, Pubescence; 2d, Pregnancy; 3d, The puerperal state; 4th, The climacteric period; and, 5th, The senile period: and the morbid conditions of the body which concurrently affect the brain under three heads, 1st, The Diathetic; 2d, The Toxic; and, 3d, The Metastatic.

Gentlemen, If this course of lectures extended twice its prescribed length—if I had to deliver twelve instead of six lectures—my difficulties would be lessened by one-half; for were the time more extended I should be able to discuss in more thorough sequence the pathogenesis, the symptomatology, the morbid anatomy, and the treatment of insanity. As it is, the long hiatus of twelve months between the present and the next course necessitates the concentration of our energies on one particular point, notwithstanding that there are others of equal interest. As I have already indicated, it is to pathology that we will mainly direct our attention, and if this be done to the prejudice of symptomatology and therapeutics, it is only for the moment, or, to speak more accurately, for the year: for next session these are the points which I propose, amongst others, to descant on. Of course they must be alluded to immediately, but only as they bear upon the question of pathology.

The terms mania, delusion, monomania, melancholia, dementia, dipsomania, and others, which have been used to indicate forms of *mental disease*, will be employed to indicate merely *mental symptoms incident upon certain conditions of the body*. It is needless now to enter upon a discussion as to the propriety of this step, more especially as it must be taken up when we come to speak of the principles of classification; suffice it now to say, that to my mind these terms have no reference to the bodily condition, history, age, diathesis, or idiosyncrasy of the individual, and that two or more of these terms may apply to the same individual within a very short space of time, and therefore have no right to be spoken of as diseases. In the whole of psychiatric medicine there are only some three or four conditions which have claims to the position of pathological entities; for instance, general paresis, idiocy, cretinism, syphilitic, and perhaps rheumatic and gouty insanity; and you will oblige me by bearing in mind, that when these terms are employed in association with those merely symptomatical, it is not right to reduce them to the same level.

You will notice that amongst the idio-encephalic conditions, I have placed last on the list that group which apparently should be considered first, viz., the congenital defects; and for this reason, that certain of these defects are so closely associated with the developmental conditions of insanity, as to render it highly desirable that their study should be undertaken in as close juxtaposition as possible. I would also point out that inflammations of the meninges and brain substance are not taken into consideration under this class, as I can adduce no evidence of their being idiopathically affected.

By *Traumatic causes*, I desire to indicate simply those produced by direct violence to the head. Attempts have been made to associate with them sunstroke, but there is not the slightest ground for the union.

It is believed, and I think rightly, that one very common cause of idiocy is injury sustained by the head prior to birth or during the process of delivery. Attempts at the procuration of abortion, and the violence to which the infant's head is subjected during its passage through a contracted outlet, or by the application of mechanical aids, may produce such traumatic lesion of the brain substance as to cause arrest of its development, and consequent idiocy. That so few children brought into the world by means of the forceps become idiots, is another instance of the wonderful power of recuperation possessed by the brain: but it is well worthy of remark, that statistics show, as far as statistics can illustrate such a complicated question, that there is less danger to the future psychical health of the being so assisted into the world, than by leaving it to the unaided efforts of nature to procure its passage through an abnormally contracted pelvis.

It must have come within the range of the experience of most present to observe how very frequently extensive injuries of the brain and calvaria are unaccompanied by permanent psychical symptoms: on the other hand, it must equally have come under your observation how apparently slight injuries have resulted most seriously to mental health. I need not recapitulate here cases which are, so to speak, historic; reference to the works of Pott, Morand, Hecker, Ogle, Ricker, Gross, Chevalier, Le Gros Clarke, and others is sufficient. Within my own experience, I can refer to a case in which I removed considerable portions of the left frontal bone of a boy whose forehead had been wounded by the kick of a horse; to another, of a naval officer whose skull-cap was cracked diagonally by a shell, almost from one ear to the other; and to a third, in which a bullet shattered the superior portion of the right parietal bone, carrying with it meninges and brain matter—all of whom made good recoveries without any subsequent serious loss of intellectual power. In the first case, the boy got well under adverse circumstances, without a single bad symptom; he was never comatose, feverish, or inclined to vomit. In the two latter cases, very prolonged coma followed the injury, but when consciousness returned the mental faculties were almost as good as they had ever been, with the exception that concentration of attention on any particular or difficult subject for any length of time produced intense headache, that the temper became somewhat irritable, that the memory was slightly impaired, and that one glass of wine caused singing of the ears and a sensation of numbness about the head. In all these three cases the discharge was very considerable from one side of the brain.

It has also been my lot to meet with several cases in which simple concussion was followed by very serious consequences to the mental faculties of the patient; also with one case in which simple depressed fracture of the occipital bone resulted in a condition similar to general paresis. Of the cases of simple concussion, two were caused by railway accident; the immediate symptoms were not severe, headache and sleeplessness being the principal ones; but on these gradually supervened inaptitude for business, loss of memory, unfounded suspicion, and a tendency to undue taking of stimulants. These two cases were almost identical in their symptoms; they both slowly recovered; many months, however, lapsing before the cure was complete. Judging from published cases and my own experience, it would seem that injuries of the head in which the skull has been fractured and hernia cerebri has occurred, are less likely to result in permanent insanity than those in which no lesion of the scalp has existed. It cannot be said that this depends on the greater immediate mortality of the two kinds of injury—at least, if we exclude from the category the slighter cases of concussion, the symptoms of which pass off quickly,—for, taking cases of fracture and of injury without fracture relatively as regards the severity of their causation, it will be found that they are very nearly equal as to their results in immediate death, but that they are not relatively equal as to the subsequent permanence of their results to psychical health. The reason of this is not far to seek. In the compound fracture, the extravasation of blood and the products of inflammation find free exit: in the simple fracture we have a permanent cause of irritation, either from depression, from spiculæ, or from the so-called *vis medicatrix naturæ*, which kills by the production of plasms which can get no vent.

Through the kindness of Dr Heron Watson, I was enabled to examine microscopically the brain of a young man who last winter was killed by being struck on the forehead by the buffer of a railway engine; he was rendered comatose, and died in eighteen hours after the accident. The calvaria and scalp were uninjured; dissection showed nothing but a very small extravasation of blood in the medulla; various portions of the brain were hardened in chromic acid, cut, and set up in the usual way. Sections of the occipital lobe showed thin extravasations of blood between the pia mater and the brain substance, due to rupture of the minute arterioles passing inwards from that membrane. Specimens from this case are now on the table, from which it will be gathered that these apoplexies were so thin as to be unrecognisable by the naked eye—in fact, that they could not have been observed except *in situ*, as the stripping off of the pia mater would have removed them along with it. The vessels of the subjacent brain substance were engorged with blood, and numerous small apoplectic clots, varying in size from $\frac{1}{25}$ to $\frac{1}{10}$ of an inch in their greatest diameter, were found in the external layers of gray matter. In the specimens before you, these are represented by *lacunæ* or open spaces, from which the coagulated blood has escaped during the process of setting up. This case presents, to my mind, points of important pathological interest; it is true it stands by itself; but it was a commonplace accident, and the appearances presented by naked-eye examination of the tissues were of the commonplace unsatisfactory nature. It is the microscopic apoplexies in the brain substance and those between it and the pia mater which demand especial attention and interest—an interest which extends to injuries beyond the encephalon. I allude to injuries of the spinal cord from concussion. Such lesions of continuity as rupture of the vessels must of course impair the nutrition of the subjacent nerve tissue, and as a consequence are followed by paralysis of function. In the brain, coma is the result—that is to say, paralysis of its function; and unless the injury is so far circumscribed that the function of the affected territory can be quickly taken up by corresponding portions of the opposite hemisphere, death is the result from the depressent effects on the general system. But the white matter of the spinal cord does not possess the same power as the brain of casting on the corresponding organs of the opposite side the exercise of functions temporarily or permanently impaired in one hemisphere. Rupture, accordingly, of the small nutrient arteries within the layers of the pia mater or at their entrance into the nervous substance must produce impairment or obliteration of the function of the neighbouring parts of the cord, which, in consequence, either undergo further degenerations, greater or less in degree according to the severity of the causating lesion, or gradually recover as the close anastomosis of the capillaries supplements the work of the injured nutrient branches. In this consists, I believe, the pathology of railway spine.

The psychical symptoms which are produced by traumatic injuries of the head are various in kind. Profound dementia of a permanent character is occasionally although rarely met with; this probably is due to numerous small apoplexies in the outer layers of gray matter destroying the relation of the cells. As has been already said, symptoms allied to general paresis have been known to ensue;—this from chronic irritation. Epilepsy with its concurrent psychical symptoms is likewise a frequent sequence. But the majority of such cases are symptomatized by change of character and the exhibition of morbid impulse. I will only allude to two: the first that of an officer whose action in the field went far to influence the result of one of the most important Crimean battles; he was promoted on the field: some years after, when in a position of trust, he met with an accident by which he was precipitated from an inconsiderable height on to his head, which fell on loose sand; he was stunned at the time, but soon after evinced a change in his *morale*, gradually became a hopeless dipsomaniac, and eventually committed suicide. Dipsomania, it may be mentioned in passing, is a very common form of insanity under this condition. The second is reported by Dr Charles Skae, of Ayr, and will be found fully detailed in the January number of the *Journal of Mental Science* of this year. The following is a short summary:—A collier was struck on the head by a mass of coal, which produced a depressed fracture of the skull three inches above the left eye: he gradually became altered in character, epileptic, maniacal, and dangerous, having attempted to kill his wife. He was removed to the Ayr Asylum, where Dr Charles Skae caused the operation of trephining to be performed over the seat of the depression: the operation was successful in every way; the epilepsy ceased, also the manical outbursts, and in a few weeks the patient became perfectly sane. This is an important and suggestive case, not only in its bearings on pathology, but also on practice.

I am loath to part from this interesting subject, but time compels me to push on to others as important.

Adventitious Products.

By the term adventitious products, it is meant to indicate all forms of tumours of the brain, of exostoses of the calvaria, and of bony deposits on the membranes. Doubts have been expressed by certain authors, as to whether tumours of the brain are really productive of insanity, their objections being founded on the fact, that in certain cases tumours have existed which have not been accompanied by abnormal psychical symptoms, without reference, however, to the position of these growths, or to certain points in cerebral physiology, which might have assisted them to explain the position. But I think no one present doubts the fact, that tumours of various descriptions are absolutely productive of insanity, notwithstanding the variety of symptoms by which their presence is evidenced, and the occasional non-confirmation of diagnosis by post-mortem examination. The frequency of occurrence of tumour of the brain amongst the insane is, judging from the experience of many observers, very varied. Writing in 1872, Dr Clouston says:[1] "At the Carlisle Asylum we have performed 214 autopsies, and there have been 6 cases of tumours of the brain, which is at the rate of 28 per 1000." In the Somerset County Asylum cases of tumours of the brain were at the rate of 16 per 1000. Dr Sutherland mentions that he found them at the rate of 20 per 1000; Bucknill and Tuke at 2·5. Fischer had not one case out of 318, and I am in the same position; for, out of 400 autopsies of insane persons of which I have records, there is not one in which tumours of the brain are recorded. I have, however, on three occasions met with gliomata in the sane.

Dr Boyd has compared the relative frequency of the occurrence of tumour in the sane and in the insane, employing the data afforded him by his experience of 1039 post-mortem examinations in the St Marylebone Infirmary, and 875 at the Somerest County Lunatic Asylum. In the former institution he found 22 cases, being at the rate of 18·3 per 1000, in which no mental derangement was observed, and in the latter 14, or at the rate of 16 per 1000. The total number of cases of tumour of the brain which Dr Boyd found amounted to 38, of whom 17 showed symptoms of insanity, which is curiously in accordance with Calmeil's observation, that one-half of cases of tumour of the brain are accompanied by more or less aberration of intellect. This estimate, however, is doubtless too high, if we include adventitious growths generally.

Of all the varieties of brain tumours, carcinoma appears the most productive of perversion of psychical function, and this is probably due to the great rapidity of its production. In the more slowly formed tumours the nutrition of the brain is not so much interfered with, and the severity of local irritation is diminished, and further, time is given for the portions of the opposite hemisphere to take up the function of the implicated regions. It very frequently happens that tumours at the base of the brain are not accompanied by any indications of insanity; on the other hand, those of slow growth occasionally produce changes in the relations of the superior convolutions of such a serious character as to necessarily implicate the mental faculties; for by pressure "they cause portions of the gray and white substances of the convolutions to pass through small openings in the dura mater, to embed themselves in the cranium, and so form true herniæ of the brain." I quote from a most interesting paper by Dr Clouston, which is the best monograph on the subject extant;[1] it has only one fault, that it is not yet finished. The following are the conclusions he has come to, as regards the pathological influences exercised by tumours growing in the brain. He says that they have three distinct effects on the brain structure.

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[1] Journal of Mental Science, vol. xviii. p. 153.

"1st, They create an irritation tending to ramollissement in the nerve substance, with which they are in contact from their first appearance. 2d, They cause pressure on distant parts, which in its turn, causes an alteration of the structure and nutrition. 3d, They set up progressive disease, and degeneration of certain parts of the nerve structure, the true nature of which is as yet not very well known; but it seems to be in some way directly connected with the essential nature and constitution of all sorts of nerve substance, whether cells or fibres. Its results pathologically are—an increase of the connective tissue in the form of granules, and enlargement and thickening of the coats of the bloodvessels; but all these seem to be secondary changes."

Some diversity exists between the observations of the various physicians who have written on this subject as to details, but they all seem pretty well agreed as to the leading characteristics of the mental phenomena. These appear to be irritability and loss of self-control, perverted morale, gradually increasing dementia; and as in the case of the traumatic influence there is an absence of delusion, and a tendency towards morbid impulse. In many, symptoms allied to those of general paresis present themselves. Dr Clouston states "that irritability and loss of self-control, and a change of disposition, are the first mental symptoms of those tumours of the brain which directly produce morbid psychosis; that the depression present seems to result from the patient's knowledge of his probable incurability, and is therefore natural: that a blunting of the whole of the mental faculties soon comes on, and gradually passes into coma: that there is a distinct and strong analogy between the symptoms, mental and bodily, produced by such large tumours, and those of general paralysis, which is the type of progressive degenerative diseases of the nervous system, inasmuch as it affects the brain and sympathetic ganglia and retina: that such cases would seem to hold an intermediate place, so far as mental symptoms are concerned, between acute inflammation of the cortical substance and blood poisonings on the one hand, and hereditary insanity on the other, the mental characteristics of the three being represented by delirium, irritability, and delusion respectively."

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[1] Loc. cit.

It is worthy of remark, that a general resemblance exists between the psychical symptoms of those mentally affected as a consequence of tumour and of traumatic injury of the brain. In both there is, as a rule, an absence of delusion, a more pronounced tendency towards morbid impulse than is observable in any other form of insanity, with the exception, perhaps, of that associated with epilepsy, and to a gradual decadence of the general system—all pointing, however, to one common cause, perverted nutrition, with its sequential lesions.

Remarks on syphilomata and tubercular deposits are deferred until we consider the diathesis with which they are associated.

But apart from these manifest tumours, we have a very important series of conditions implicating the connective tissue of the brain, which can only be demonstrated under the microscope. I allude to various forms of sclerosis of the neuroglia, to colloid degeneration of its nuclei, and to the morbid bodies which we call amyloid.

Under the head of Over-Excitation of the Brain, we will consider what are generally called the *Moral Causes of Insanity*. In systematic works and asylum reports the causes of insanity are usually divided into two great classes—the physical and the moral; the former being referred to conditions of the body, the latter to conditions of the mind. We will shortly weigh the arguments *pro* and *con* for such a differentiation, and inquire how far and to what extent the so-called moral is in fact a physical cause.

It is not for me to remind you, that emotion acts as an inducing morbific influence on organs remote from the brain. Under certain circumstances the trophesial or nutritive influence of the central organ is exercised with extreme rapidity on remote structures by, we believe, its power over the vasomotor nerves. Within the confines of health we see this influence manifested in the blush of shame and the pallor of fear, the goose-skin of terror, and the diuresis and diaphoresis of an emotion, compounded of anticipation, doubt, anxiety, and fear. Beyond the pale of health we have, amongst many other changes caused by the influence of emotion, the perversion and arrest of milk in the nursing woman, and the diarrhœa and paralysis of fright. I was once witness of the latter phenomenon in a very marked degree. A soldier belonging to a distinguished regiment, with which I once had the honour of serving in a temporary official capacity, was tried for striking his superior officer during time of war. He was a strong muscular man, in good health, and had proved himself a brave soldier in action, although a bad one in time of peace. He was tried by court-martial. The regiment was formed in square to hear the finding of the court and its confirmation, and to witness the carrying out of the sentence: the verdict was guilty, and the sentence that "Private A. B. shall be shot to death by musketry." Private A. B.

dropped as if the sentence had been actually carried into effect: he had not fainted, but had lost completely the use of his legs: he was supported till the rest of the proceedings were detailed. These were, that, in consideration of certain circumstances, the sentence was commuted to forty lashes and penal servitude. As soon as A. B. realized the fact that his life was to be spared, he regained all his faculties, walked to the triangles, which were presently rigged, stripped, and took his punishment like a man, well laid on as it was, without a groan, and marched to hospital when it was over.

Proceeding higher in the pathological scale, we occasionally meet with cases in which more permanent and serious bodily disease is induced by sudden emotion. I will instance only one case—one, however, which doubtless will bring to the memory of each and all of you instances of a similar nature. A lady, who had been confined of her fourth child, all her previous confinements having been of the easiest nature and shortest duration, was, when all was going on quietly and comfortably, suddenly informed that a beloved sister had died in childbed: she immediately had a rigor, and in three hours symptoms of peritonitis set in with great intensity. No symptoms of puerperal insanity showed themselves; she was comatose for many days, was brought to death's door, but eventually pulled through, and is now the happy mother of children, another having been borne with the utmost ease since the confinement spoken of.

Emotion may be the cause of death. I do not speak of those cases of sudden death from joy or sorrow which are on record, as we have not their clinical history, without which they can have no pathological value. You will find, in a work called "Old New Zealand, by a Pakehah Maori," a description of rapid death from chagrin, as occurring amongst the natives of those islands. I was witness of two such cases, but will relate only one, as they were almost identical. A strong healthy young man, belonging to a well-known native family, became, without any known cause, disgusted with life: he had no indication of any bodily disease, refused food, but not liquids, and did not attempt to commit suicide. He took to his bed, and in less than a week after the first manifest symptom was found there dead. Such cases are not uncommon amongst the South Sea Islanders.

It would be beyond my province to endeavour to explain here the *modus operandi* of emotion in producing morbid changes of remote organs, more especially as the subject has been carefully investigated by many authors, more especially by Dr D. H. Tuke, whose interesting book on the "Influence of the Mind upon the Body" is well worthy of study. It is for us to seek an explanation of disease of the brain, and the physical changes which take place in consequenee of prolonged or undue emotional action.

I have already stated that Ferrier's experiments show that the stimulus of Faradization causes a determination of blood, a functional hyperæmia in the part of the brain submitted to its influence, and that certain phenomena are consequent thereupon. Of course it is and may always be incapable of absolute demonstration, that the same condition exists in parts stimulated by the exercise of their intellectual, volitional, or emotional functions; we can only argue that such is the case from extraneous evidence. Every man in this hall has experienced the sensation of fulness and throbbing of the temples and forehead, accompanied by coldness of the feet and legs, after prolonged and absorbing study, followed by sleeplessness and headache. No one chooses the hour immediately succeeding dinner for active thought, or, if he does, he does it to the detriment of the process of digestion. It is needless here to produce the various witnesses at our call in support of the opinion, that when the blood-supply is increased, activity of the brain is produced, and that it is diminished, *pari passu*, under opposite circumstances. The analogies of other organs afford correlative evidence.

But functional hyperæmia, which is in itself normal so long as it is requisite, becomes productive of abnormal results if it is continued longer than the occasion requires, as it causes unnecessary activity of the supplied tissues, and consequently unnecessary prolongation of the exercise of their function, and prevents a condition necessary for repair—that is to say, the anæmia of sleep. Again, blood too suddenly and in too great quantity determined to certain portions of the brain by the stimulus of emotion, may produce explosive symptoms by immediate over-stimulation of the cells. We have thus two abnormal conditions of blood-supply—the first in degree chronic, the second in degree acute. The chronic, far the more common, is the immediate cause of loss of sleep, with its manifold sequential symptoms; the second, comparatively rare, causing immediate over-excitation of the cells, and perversion, temporary or permanent, of their function.

The investigations of Durham, Hughlings Jackson, and Hammond, have definitely determined that during sleep the supply of blood to the brain is materially diminished; that, in fact, during sleep the brain becomes anæmic, and the rapidity of the flow is materially modified. Against this may be instanced the stories told of active brain-work carried on and completed during sleep; of problems solved, and sequential trains of thought worked out in dreamland: these are mere statements, unsupported by aught but supposed experience. Those who have believed that they had undergone such experiences were either not asleep, or the results they supposed they had obtained occurred during the minutes or moments of wakening, when the blood was acting on cells already stored with the factors of sequential thought. Such stories hold no position in scientific pathology.

If you will each and all analyze the cases of insanity which you have ascribed to undue emotion, I believe you will find that a very small proportion indeed are referrible to it as the *immediate* cause: I mean, that deliberation will show that a considerable interval has occurred between the emotion and the first indication of absolute mental alienation—an interval during which sleep has been absent in consequence of continued hyperæmia, primarily caused by emotional influence. I have appealed to the experience of many physicians who have devoted their lives to the study of mental disease, as to the relative proportion of immediate and sequential cases which have come under their observation. I will refer you to the experience of one such physician, Dr John Smith, of this College. His experience, which extends over half a century and many thousand patients, and which is supported by accurate written records and a most retentive memory, can instance no well-marked case in which insanity has been proximately caused by emotion: he has known of several in which the allied symptoms of epilepsy and catalepsy have been produced by sudden fright, but the search of records and the taxation of memory can produce no cases in which mental aberration, more or less persistent, has resulted. I speak without the book, but within the mark, when I say that you will find that cases the immediate results of emotion stand in no higher ratio than 1 to the 10,000 of those in which mental symptoms show themselves, as the result of loss of sleep induced primarily by emotion. Within my own experience, I have never met with an instance. Of course you will understand that such complications as pregnancy, the puerperal state, or lactation, are not involved in the present discussion.

The rarity of the immediate causation of insanity by abnormal explosion of emotion is all the more curious, when we remember the comparative frequency of its morbid influence on remote organs. It would appear that great provision is made for the over-exercise of function of the cerebral tissues, and that they are less liable to sudden morbid change than the parts from which they receive and to which they convey influences.

Although the limits of cerebral health are broad, they can be overstepped by the withdrawal of the condition of sleep, during which the brain tissues are rested and repaired. Disappointment from reversed fortune; the carking care of the speculator in love or business; the anxiety of the fond husband, father, or mother, watching at the sickbed; the retrospection of the mourner on the days that are no more; the weariness of anticipation; doubt and hope deferred; the despair caused by the austerity of certain forms of religious beliefs, their plenitude of damnation and their straitness of salvation—are but a few of the so-called moral causes which induce loss of sleep. *They are not moral causes, they are absolutely physical.* They bear but slight analogy to the causes of degeneration of the tissues of other organs, inasmuch as they take their rise in the normal exercise of the functions of the cells, which, passing beyond normality, results in certain physiological modifications of blood-supply necessary to health: in this consists the difference between emotion and such morbific influences as cold, heat, and septic or other poisons.

Over-exercise of the intellectual function is not by any means such a prolific cause of insanity as undue emotion. It is not work but *worry* that kills the brain. When both are combined the result is often rapid. Take the case of Sir Walter Scott: how hard he worked, and how happily he worked, in the earlier part of his literary career; but when he was compelled to labour and tax a brain, already overtaxed by worry and anxiety, how fearfully rapid was the breakdown! A converse case was that of Whewell, who, in the calm atmosphere of the academy, worked to the last day of his life, with but slight modification of his intellectual power, notwithstanding that evidence was found after death of brain atrophy. The forced intellectual work of Scott was a very different thing from the automatic cerebration of Whewell. The exercise of the imagination to supply under adverse circumstances ever-called-for "copy," bears no relationship to the outcome of a brain trained to systematic ratiocination—a brain from which flowed without compulsion, almost without volition, the results of the stored-up impressions of years: the brain of the one succumbed to work and worry; the brain of the other retained its intellectual faculties to the last, those tissues only being affected by senile decay, the degeneration of which does not imply loss of thinking power. Scott's cells underwent degeneration from loss of sleep; Whewell's maintained their continuity from the due recurrence of repose.

In the absence of the anæmia of sleep, the over-stimulated cells give off their outcome in gradually increasing intensity, till the patient goes mad, possibly from the loss of inhibitory power. The primary emotion as a rule, though not always, influences the characters of the excitement, which is generally of short continuance, leaving the patient weak and exhausted. Unless relief is soon found, the changes in the cells are accompanied or followed by lesion of other brain structures, which are productive of most important pathological conditions affecting the general system, which at times result rapidly in death. These will be considered separately: they are only referred to now to remind you that there is no change in cerebral tissue which has not its effect on the body generally.

I trust you will not think that this particular subject has been dwelt on too long, for I believe it is one of the few keys to the general position. It has, moreover, most important bearings on the theory and practice of psychiatric physic.

We now pass to the last of the idio-encephalic conditions productive of insanity.

Defective Organization.

In the wide domain of medicine, we are constantly compelled to ascribe certain trains of diseases to a defect or defects in organization, pathological conditions the nature of which have not been elucidated by the various scientific instruments at our command—conditions, however, which are easily recognised by the *mens medica* of the practised observer. We are accustomed to speak of these as neuroses, diatheses, or tendencies, words implying an instability of relationship of the constituents of the different systems in which the trains of diseases are manifested, which renders them peculiarly liable to abnormal nutritive changes, and consequently to perversions of function. Is this assumption of a general tendency towards degeneration a cloak for our ignorance? I think not—it is only an assertion of the amount of knowledge we at present possess. We can fairly point out the ætiology, the pathogenesis, the symptoms—in a word, the general pathology of these conditions. They are entities, save in one thing, their histology. We cannot lay our finger, or rather I should say our eye, on the defective elements. What is true of the other systems of the body is peculiarly true of the nervous system. We cannot say in what nerve element or elements the defects primarily occur. We have no data to go on in ascribing nervous instability to a deficiency in bulk of a particular constituent, to an impairment or increase of conductivity of nerve fibre, to a deficiency or over-activity of chemical action of cells. I am not pleading *ad misericordiam*, when I say that instability of the nervous system, neurosis, diathesis—call it what you will—is in no worse plight than the other well-marked morbid general conditions to which you apply the several terms, in accordance to the various systems through or by which you regard pathology. It is no disparagement to neuropathy to admit that the nervous system is liable to conditions of instability which have not, and probably can never be, submitted to ocular demonstration. But it would be feeble to cast aside as useless from an imperfect science a broad generalization, founded on the observation of all, simply because one link was missing in the chain of evidence.

To enter upon a full description of the natural history of this constitutional peculiarity is quite uncalled for now, as every one present is well acquainted practically with the subject. I only ask you to remember that it is claimed as a very important idio-encephalic condition, productive of insanity. I refer those who desire to become acquainted with the best writings on this question to the works of Maudsley, Bucknill, Tuke, and A. Mitchell, where the various interesting points connected with its heridity, its influence on the morale and the intellect, and the corporeal frame, are fully discussed. I am free to confess that here is the debatable land on which the psychologist and the pathologist meet on the fairest terms; for they both are equally armed with hypothetical weapons, neither of them possess the lance of absolute demonstration;—the only difference being that the psychologist *thinks* he has it, and the pathologist *knows* he has it not. As one man may be tubercular, and yet not suffer from tuberculosis, as another may be gouty without developing what we call gout, so a third may be constitutionally predisposed to nervous instability without ever becoming insane. We specialists await the incidence of manifest symptoms of insanity, as you do the indications of phthisis or podagra, before we assert any absolute pathological rights.

ARTICLE III.—*On Local Hygienic Influences.* Part II. By JOHN BOYD, M.D., Slamannan.

IN a former paper we had occasion to refer to the parish of Slamannan as a type of the second region into which the Lowlands of Scotland are divisible, and mentioned the social and industrial revolution brought about by the formation of the Monkland Railway, which, as readily can be understood, introduced a total change of parochial circumstances from the time previous to that event, when two old women carried up weekly on their backs from Falkirk all the wheaten bread consumed in the district; and when two joiners from that locality raised a disturbance because the mistress of the solitary hostel would not rescind in their favour her laudable rule of selling only ℥iv. of ardent spirits *per diem* to each individual. The special constables were called in, the culprits apprehended and taken to the local J.P., who, after hearing evidence, said he would inflict a very severe sentence for the heinous transgression of causing a breach of the peace in a quiet community, and would fine them in a sum sufficient to support all the poor of the parish for six months. Accordingly he ordered them to pay half-a-crown between them, and to take the road to Falkirk directly.

The mineral treasures abounding in the district being thus opened up, an entirely new population became gradually introduced, soon outnumbering the natives.

These minerals consist of various seams of coal about two feet in thickness, and varying in distance from the surface from twenty to forty fathoms—in one instance about sixty fathoms. There are strata of shale and slaty-band ironstone, but as yet these have been little wrought; while the coal-beds, under their various designations of Johnstone, Ladygrange, splint, and soft coal, though thin, are of first-class quality, commanding the highest price in the market, from cleanness and durability in burning, absence of dust from their heavy reddish ash, and the suitability of the hard splint coals for coking purposes. In addition to this, in several of the fields the interjection of trap-dykes and subjacent sheets of igneous rock semi-calcining the fuel, renders it remarkably well adapted for raising steam in locomotives, marine engines, and steam-power engines generally. The constant demand for these special carbonaceous products gives ample employment to a large and increasing body of workmen, who, labouring on these thin seams on the *long wall* system—that is, undermining and extracting the *whole* coal in a continuous surface—require rather more skill and experience than is necessary for the underground quarrying of the *high coals* about Wishaw, etc., where the *stoop and room* system prevails.

Access to these strata is gained by excavating pits twelve feet by five down to the necessary depth; these are lined with wood, and divided by a close partition into two shafts. At the bottom, from where the roads into the workings branch off, a fire is kept in a sort of furnace termed a *cube*, which, by heating and rarefying the air, causes an upward current in the one compartment, producing a downward draught in the other, which is led by appropriate measures along the faces, and the pit is thus ventilated and noxious gases carried away. In our district we very rarely meet with instances of explosions caused by *fire-damp*—that mixture of light carburetted hydrogen CH_4, and olefiant gas C_2H_4, which has produced so many disasters in South Britain. Whether it be from the less quantity of carbonaceous matter in thin seams, or from their comparative shallowness, that fire-damp explosions occur so seldom here, it is difficult to decide: the latter view is supported by the circumstance that what slight instances do occur are almost wholly confined to two of the westmost pits in the parish, which are the deepest, being about sixty fathoms each, and from their depth, nearer the central heat of the earth, more elevated in normal temperature, and thereby more apt to evolve the gaseous emanations referred to.

Immediately above the freestone which forms the roofs of the coal-beds, a slabiform, argillaceous and arenaceous compound called *faik* is frequently found, from which oozes out the *white choke-damp* or carbonic oxide CO, which is known, not by extinguishing the collier's lamp, but by elongating the flame into a tapering bluish pencil; and this may deceive, and often has done so, those who were not instructed as to its noxious qualities. From the clayey *daugh* or pavement which constituted the subsoil of that tropical vegetation which, in its decay and submersed compression, forms our stores of fuel, arises the *black choke*-damp or carbonic acid, CO_2, which extinguishes flame and respiration, and renders many a passage in disused coal-workings a veritable *grotto del cane*. It has not been my lot to see many fatal accidents from this cause. In some of the southern and western districts such cases are not unexampled, and whenever a miner is overcome by this gas, I am informed the remedy there adopted is to bury the sufferer in the earth, all but his head, and it is stated that this *erdbad* generally produces complete restoration in a few minutes. Be this as it may, the workmen here frequently apply for relief from dyspnœa and bronchitic symptoms, which they ascribe to a *dose* of *damp*. In such cases the administration of an emetic, a cholagogue cathartic, and semi-stimulating expectorant, such as .the mistura ammoniaci, I usually find productive of satisfactory results.

In addition to these, it may be stated that when stagnant water lies long in the underground pathways, in course of time it exhales sulphuretted hydrogen. This, along with the coal-dust let loose by the mechanical action of the workman's pick at the coal face, the smoke evolved by the combustion of the train-oil that feeds his lamp, added to the various noxious damps above mentioned, must irritate his air-passages, impede the oxidation of the venous blood in the capillaries of the ultimate air-cells, and aggravate the unwholesomeness of his employment, notwithstanding every available precaution that may be, and almost universally is, adopted. In discussing the special diseases to which, from the nature of their employment, hill-people or miners are exposed, Franz G. Kornatowsky says:[1] "Auf Bergleute stürmen so vielerlei schädliche Einflüsse ein, dass es nothwendig wäre, für sie besonders ein populäres Werk zur verfassen, worin sie mit einigen Gesundheits verhaltungsregeln bekannt gemacht würden . . . . Mangel an reiner atmosphärischer. Luft, die mit ausdünstungen aller Art verdorben ist z. B. entzündbare Schmiede und steinkohlen dämpfe, stinkende mephitische Dünste, Näse u. dgl. erregen Empfindungen von Kalte, Ohnachten, stören die Verdauung, Lungenschwind suchten, asthenische Brust-beschwerden, Asthma, nicht selten entsteht plötzlicher Tod.

"Die verschiedenen Lagen, Bewegungen und Anstrengungen des Körpers solcher Bergleute, sind gewiss mit zu den zwar unvermeidlichen schädlichen Einflüssen zu zählen, u. s. w."

The following passage in Dr Copland's "Dictionary of Practical Medicine," article Arts and Employments, vol. i. p. 124, is so much to the same purpose, that it might also be taken for a free translation:—"Working in constrained positions shows its effects most decidedly in *colliers* and *miners* who labour chiefly in the sitting or kneeling posture, frequently with the body bent in the greatest degree, in an unnatural atmosphere, frequently containing hydrogen, or carburetted hydrogen, and carbonic acid gases, and with artificial light. They are moreover exposed to changes of air, and occasionally work with their feet in water. They are generally spare men, with slightly curved spine and bowed legs. When the dirt with which their skin is usually loaded is removed, the complexion seems sallow and unhealthy. Their complaints are, asthma, rheumatism, disorders of the head, intolerance of light, etc., evidently resulting from the circumstances just stated connected with their employment, and their exclusion from the beneficial influence of sunshine, light, and air. They are not generally very intemperate, yet they seldom live beyond fifty."

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[1] Kunst Krankheiten genau und gründlich zu untersuchen. Koburg und Leipzig.

The mining operations in this section of the muirlands have not as yet extended beyond the duration of one generation, and although the cases of premature decay—of emphysema of lungs, spurious melanosis, chronic bronchitis, with the consequent pulmonary and hepatic congestions, dilatations of the right side of the heart, asthmatic paroxysms, and the ascites and anasarca which usually terminate the unfortunate victim's sufferings—have been too frequently observable, yet these are invariably importations from other quarters where the workings are deeper, farther extended, and therefore more pernicious. Whether from the bracing external atmosphere, shorter hours of labour, better nourishment from higher wages, improved house accommodation, and better education, there has grown up among us a vigorous and purpose-like male population, who exhibit in many respects a marked improvement on their predecessors. No doubt there are too many who degrade themselves and do all they can to deteriorate their constitutions and circumstances by self-inflicted alcoholic narcosis; but, taken as a whole, they do no discredit to the operative community. While passing recruits for the Stirlingshire Highland Borderer militia regiment, I observed that the youths from Slamannan measured in thorax circumference from thirty-three to forty-one inches; while, in other parts of the county, the greatest chest girth was from thirty-one to thirty-five inches. A gentleman on the staff of that regiment remarked that the lads from this quarter were much approved of for respectable appearance on joining, and quiet orderly demeanour during their annual drill.

Notwithstanding the improved regulations for ventilating and inspecting mining operations, it still remains and always must be a dangerous and unwholesome employment. In addition to the perils of the perverted atmosphere already discussed, the attraction of gravitation is always at work, seeking, so to speak, every chance to crush the collier between the roof and the pavement at his working face. A piece of coal several feet in length and a ton or so in weight, when undermined, may come down unexpectedly soon, and destroy his body or limbs. A mass of freestone may drop from the roof with little or no warning, and extinguish life in an instant. It is true that by new arrangements, pit-props or *trees*—*i.e.*, pieces of wood from three to three and a half feet long—are now cut to their hand and brought forward to the working places, so that every doubtful part can be speedily and easily secured, and thereby rendering accidents from this cause much less frequent than they formerly were when familiarity with danger often induced culpable carelessness, yet still they do and must occur. A scuttleful of coals, quite as much as a basket of Newhaven fish, involves the lives of useful men; and a collier descending a pit, like a slater ascending a roof, is much on a par as regards safety to life and limb with a soldier going into action.

From lying extended while at work this class of men are peculiarly liable to fractures of the inferior extremities. Although there is no necessity to kill a horse every time a pit mischance occurs—and it is well to bear in mind that generally some one is ready to run for the doctor before it is ascertained that there is any particular need for his services—it is highly advisable for him to be on the spot as soon after as he possibly can. There is no call for delay while looking out elaborate appliances. The first concern is to identify the exact nature of the injury; and, if it be a fracture, the requisite apparatus is easily improvised. Wherever there are houses, there are women: wherever women are, there are sure to be bandboxes. One of these cut into requisite lengths, and three and a half inches broad, *threefold*, fulfils every necessary condition as regards lightness, cleanness, strength, and elasticity; and the fair sex, with that ready sympathy for suffering humanity to which each one of us is so much indebted at all ages and seasons, are ever ready to sacrifice their spare linen on such occasions, so the medico is amply supplied with an abundant stock of rollers and bandages.

Fractures of the humerus, the radius and ulna, the tibia and the fibula, may thus be provided for out of the bandbox; when the femur sustains a solution of continuity, the joiner attached to the work will produce a Dessault's long splint, with short instructions, and in as short a time. A weight attached to and depending from the foot is a valuable supplement to the extension action of this appliance.

One very important consideration not to be overlooked is the remarkable tolerance of injury among this class of men, so that conservative surgery can be more freely attempted and carried out among them than almost any other division of the community. In support of this proposition I may adduce the case of J. R., æt. 28, who, in July 1873, sustained a compound fracture of the tibia and the fibula at the junction of the middle and inferior thirds, by the fall of a *kettle* (the large tub by which the excavated materials are hoisted from the bottom of the shaft to the pit-bank) in a pit three miles westwards, then in process of sinking. Happening to be in the vicinity of the scene of the accident, I saw him as he emerged from the cage and accompanied his removal to his house, a quarter of a mile distant. On examination the crepitus of the fracture was felt, the displacement visible, and two large wounds anteriorly and posteriorly, besides a more lacerated one over the phalangeal-metatarsal articulation in the plantar aspect. The foot and ankle were devoid of sensation, the circulation nil; so my first impulse was to send off for instruments, chloroform, and professional assistance, to amputate immediately. After he was undressed, washed, and put to bed, the circulation and sensibility returned to the foot. I thought it worth venturing to retain the member; brought the wounds close by interrupted sutures, adjusted the fracture, applying one posterior and two lateral splints made of bandbox, having previously bandaged from the toes upwards. This done, I got a sling made of a yard of strong unbleached cotton cloth eighteen inches wide, sewed together at the ends, and fastened by a cord to two stout nails inserted crossways into the board above the bedside to what the curtains are attached. Into this the fractured limb was inserted. The object of this homely modification of Salter's swinging cradle was to obviate the tendency to displacement that is unavoidable when the injured member is laid in the bed, especially when the patient is asleep or just awakening. A professional friend who saw him the same evening with me, recommended the packing of the splints with oakum, which was done, and the antiseptic treatment by carbolic acid, oil, and lotion, carried out as completely as circumstances would admit. Suppuration was scanty; the anterior and posterior wounds healed in ten days; that below the toes in fourteen, and the osseous union proceeded normally and satisfactorily. J. R.'s health remained unexceptionally good. By the end of the fifth week he got out of bed and walked on crutches *nolens volens*, and long before the close of the year had resumed his employment. It so happened that while previous to the accident his nether limbs were both somewhat parenthetical ( ), the fractured leg was rather the straighter of the two. Influenced by considerations of symmetry, his friends suggested to him to have the sound limb broken and put through a similar treatment; but he has not as yet seen fit to accede to the tempting proposal.[1]

From the muscular exertions and constrained positions involved in their peculiar labour, miners are extremely liable to sprains of the lumbar muscles and of the erector spinæ muscles generally. As when this occurs there is invariably much cutaneous and subcutaneous congestion of the capillaries, I always find the application of a few leeches and complete rest in bed for a time the most summary and satisfactory source of relief.

As a specimen of the odd cases that diversify mining practice, that of R. F. may be adduced. This person returned home *Bacchi plenus* one forenoon in April last, and was severely reprehended by his wife for thus stultifying himself and wasting their resources when a *strike* was impending. Waxing wroth at her reproaches, he resolved to chastise her; but not liking to hurt her person, he compromised by knocking his fist through a framed and glazed picture hanging on the wall, inflicting on himself a wound two inches above the wrist in the anterior aspect, exactly of the size and shape of the mouth of a two-year-old when crying at his loudest. A worthy and weighty midwife who was passing, was called in to *deliver* him; she put a bandage over the wound and sent off for me. On my arrival I undid the roller and immediately the anterior interosseous and radial arteries spurted over my face and front, giving me in a few seconds a most assassin-like appearance. This, with the patient demonstrative and pot-valiant; the house filled with the neighbouring wives, each with her baby in arms—the mothers weeping and lamenting, the infants screaming,—made a most striking tableau. But matters soon arranged themselves. All superfluous parties were sternly dismissed from the dwelling. Big Susy held down the patient's head by main force on her ample bosom; a matron barely out of her teens manifested all the coolness, intelligence, and dexterity of a practised hospital dresser. She held the orifice of the interosseous with the dressing forceps, till I tied it with a ligature; the radial, previously caught by spring forceps, was next secured; she brought the edges of the wound into apposition till I joined them by three sutures; lint and a roller were next applied, the arm slung, and the sufferer put to bed—a wiser and a weaker man. Suppuration was slight, in consequence of assiduous carbolic applications. The union of the tendons took some weeks, but by the end of the *stand* he was about ready to resume work; and on joining the yearly drill of the Stirlingshire militia, he was utilized for hospital or police duty, so that Her Majesty's service took no detriment by the transaction.

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[1] Another collier, who is just emerging from his six weeks' incarceration in bed from fracture of leg, has devised an improvement on the leg-sling above referred to. He got the joiner to fix a bracket to the partition at the bed-foot about four feet above him, to which the cord was attached above, while below it was fastened to the middle of a wooden cylinder 15 inches by 1½, inserted into the sling, affording a more even support to the depending member.

ARTICLE IV.—*Antiseptic Incisions an aid to Surgical Diagnosis.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

THE aspirator, exploring needle, and trochar, are all valuable aids in the diagnosis of doubtful surgical cases, but even these instruments are not sufficient to determine the exact nature, connexions, and extent of the disease in many obscure cases. We may by these means safely and surely in many instances ascertain the presence or absence of fluid, and, if present, its nature, and we may also determine whether or not a swelling is solid, semi-solid, or the result of a collection of fluid; but in certain cases of great surgical importance more information—such information as can only be gained by tactile examination of the diseased part itself—is required before satisfactory treatment can be carried out.

It is the constant practice of surgeons to enlarge sinuses and fistulous openings, so as to ascertain more accurately the nature and extent of diseased or injured structures; and it is also the practice, but to a much more limited extent, to make special exploratory incisions for the same purpose. The practice of making special exploratory incisions has hitherto been very limited, principally owing to the fear that an incision might be followed by bad results, or complicate the case. Having during the last few years made a free use of special exploratory incisions, with the important addition of careful antiseptic treatment of the wound, I have learned the value and safety of this new aid to surgical diagnosis, and I therefore wish to urge its more frequent use in important surgical affections. I would here remark, that in affections of the extremities the employment of the "bloodless" method of operating is, in suitable cases, a valuable assistance in making such investigations, for the various tissues can now by this means be seen and examined as conveniently as in the dead body.

I propose briefly to consider some of the classes of cases in which I think that these antiseptic incisions are valuable.

First. In obscure cases of suppuration.

In abscesses, or suppuration occurring in the abdominal or pelvic cavities, or in some of the important surgical regions, the presence of pus cannot always be detected by the exploring needle, and I have on several occasions discovered a collection of pus by an antiseptic incision, having previously failed to obtain this information by the use of the former means. An additional advantage in such cases is that the introduction of a finger will usually determine definitely the situation and cause of the condition; and the great importance of detecting *early* the presence of pus, so as to allow of its being removed, is recognised by all practical surgeons.

Second. In cases of obscure morbid growths, or doubtful solid swellings.

The question of treatment which arises in such cases can only be determined by an accurate knowledge of the nature and connexions of the diseased condition. In some instances careful external examination, and a study of the history of the affection, may be sufficient for the purpose, but there are other cases in which a proper diagnosis cannot be made by these means alone. In these latter cases, an incision carefully made in a convenient situation, so as to allow a finger to be introduced, will surely decide any doubtful points, and determine what treatment is most likely to be beneficial. I have in this way received valuable information in regard to the treatment of obscure cases of tumour or swelling in connexion with the jaws, groin, and other regions. For illustration, I would refer to a case lately under my care.

A young man, æt. 28, was admitted into my wards on the 5th of October last, suffering from a large swelling connected with the right side of the pelvis. Four months before admission, a small swelling had been discovered a little internal to the crest of the ilium, and this swelling had continued rapidly to increase. The patient's general health has during this time suffered much; he is becoming markedly emaciated, and has constant pain and cramp in the leg of the affected side, due, apparently, to pressure on the nerves.

An external examination of the swelling determined that its probable nature was a soft medullary cancer growing from the ilium. It was the size of an adult head, and filled up the whole of the iliac fossa, and extended upwards to the false ribs, and inwards to the middle line of the abdomen. It also showed a slight bulging in the centre of the gluteal region, proving that the ilium was involved to a slight extent on the outer as well as the inner surface. The tumour was perfectly circumscribed and free on its upper and inner aspects, but its base was firmly attached to the inner surface of the ilium.

This was the extent of our diagnosis by external examination, but it was impossible to determine accurately the extent of this attachment, and the non-involvement or involvement of the neighbouring pelvic bones.

The patient having begged me to afford him, if possible, some relief, I explained to his friends and himself the doubtful point of the case to which I have referred, and the dangerous nature of the affection, and I suggested the advisability of making an antiseptic exploratory incision, so as to decide the possibility or non-possibility of the removal of the growth. I gave this advice because I have for some time been of opinion that it was quite possible to remove successfully more or less of the expanded portion of the ilium in cases of tumour or other disease affecting and confined to this portion of the bone. Consent having been given, I made an antiseptic incision a little to the inside of the crest of the ilium, much in the same direction as that used for ligature of the common iliac artery, and after dividing the various coverings, exposed what appeared to be the capsule of the tumour. A finger being introduced, and the connexions of the growth so examined, it was found that the growth, as had been supposed, was free on all sides except at its base, which seemed to be attached to almost the whole surface of the ilium, and to the brim of the pelvis as well. Further dissection, however, showed that the firm attachment of the lower part of the base was owing to the psoas and iliacus muscles being tightly stretched over the tumour, and when these were divided the only real attachment was to that expanded portion of the ilium which admitted of being removed, together with the tumour. Unfortunately, the patient's pulse began to fail, and therefore it was thought advisable to tear away the growth from the bone, so as to hasten the completion of the operation. This was easily done without hæmorrhage, the iliac vessels which ran along the inner margin of the tumour having first been carefully separated from it, and a clamp applied over the abdominal aorta. The bone, which was found softened, and in some parts destroyed, was freely touched with the actual cautery, and afterwards with the solution of chloride of zinc, and the cavity of the wound was stuffed with lint saturated in carbolic acid. The patient revived after the operation, and his strength improved for the first four days, but on the fifth day he became weaker, and died somewhat suddenly.

Had this case been seen at an earlier period, I think that a successful operation might have been the result, the diagnosis having first been determined by an antiseptic incision; and I trust that the records of surgery will soon be able to report the successful removal of a portion of the ilium for the relief of a growth which has not been permitted to make so serious a progress without surgical interference, as in the case just related.

Third. In diseases of joints.

When suppuration has taken place in the interior of a joint, the well-recognised practice is to evacuate the pus by incision as early as possible, with the object of preventing or limiting further mischief. In suppuration of some of the deeper joints, such as the hip, it is not always possible to detect by external examination the presence of pus, and therefore surgical interference in such cases is usually delayed until fluctuation has become more distinct. But delay under these circumstances is not advantageous, and the joint structures and surrounding tissues all suffer during the progress of the suppuration, and consequently the result cannot be so satisfactory in the majority of instances as when the pus is evacuated at an earlier period. I have relieved by an antiseptic exploratory incision five or six cases of commencing suppuration of the hip-joint, and ascertained the exact condition of the articular surfaces, in which no distinct external signs of the presence of pus could be detected. Without the aid of antiseptics in these cases I would have hesitated to open into so important a joint.

A case now in my wards illustrates these remarks admirably. A girl, æt. 14, having injured her hip-joint a month previously, was admitted under my care on the 21st of August last. Her symptoms were those of very acute articular disease, but no fluctuation could be detected externally. The girl could neither move nor sleep, so severe was the pain, and she constantly cried out for relief. Feeling sure that there was suppuration of the hip-joint, I made an antiseptic incision, introduced my finger into the joint, and found there a small quantity of pus, and an almost complete destruction of the cartilage covering the head of the femur; the acetabulum was scarcely affected. I sawed off the head of the bone, and dressed the wound in the usual way. The operation gave complete relief, and in three weeks and two days the wound was soundly healed, and remains so. Those acquainted with the usual progress of such cases of hip disease will surely acknowledge that my operation saved this patient much local and constitutional irritation.

I have, however, advanced still further in the treatment of joint diseases by means of the antiseptic incisions, and have *prevented* matured suppuration taking place. A short note of the two following cases will explain this somewhat bold assertion:—

CASE 1. Mrs B., æt. 32, was seen, with Dr Williamson of Leith, on the 19th of October 1872, on account of an affection of her knee-joint. Two weeks before, symptoms of acute synovitis showed themselves in the knee, and have continued without any relief until the present time. The patient is within three or four weeks of her confinement, and the condition of her knee is causing her and her friends much anxiety. An examination determined an acute affection of the knee-joint, with considerable swelling on all its aspects. There was distinct fluctuation, but it was somewhat peculiar, and I felt doubtful whether the fluid was serum or pus. In order to determine this, I suggested a puncture with the exploring needle, and having drawn off in this way a small quantity of the fluid, I found that it resembled in appearance very turbid serum, and a microscopic examination of it discovered distinct pus cells.

A consideration of this case, and the peculiar circumstances in which the patient was placed, made me advise that the joint should be at once freely incised antiseptically, hoping somewhat anxiously that by an early evacuation of the fluid, with the aid of antiseptics, I might prevent suppuration and disease of the articular surfaces of the joint, if they had not already become affected.

The limb having been placed on a wire splint, an antiseptic incision was made on the lateral aspect of the joint, and the articulation freely opened. Several ounces of sero-purulent fluid were evacuated, and a careful examination of the whole surface of the joint was made with the finger. These surfaces being found unaffected, a drainage tube was inserted, and the wound dressed antiseptically. The tube was retained in the joint for four days after the operation, being removed daily for the purpose of being cleaned, and then reinserted at the time of dressing of the wound. A little serum escaped daily into the dressings for the first five or six days after the operation, but after this there was no further discharge from the joint, and in three weeks the wound was a mere superficial sore, and the wire splint was removed and lateral leather splints applied. A few days after, a most successful confinement took place, and within the three months after the operation the patient was going about quite well.

CASE 2. On the 14th of August last I saw, in the absence of Dr Chiene, D. G., æt. 21, on account of an inflammation of his right knee-joint. The patient had been suffering for about one month from synovitis of the joint, and had been carefully treated in the usual way. As he was making no satisfactory progress, Dr Ritchie, who was seeing him for Dr Chiene, asked me to give my opinion in connexion with his case. An examination of the knee, which was being admirably kept at rest in a suspended wire splint, convinced me that there was considerable effusion into the cavity of the joint, and that this effusion was of a very doubtful nature. The patient's general condition, pulse, and temperature, the latter of which rose to 102° every evening, all showed smart constitutional irritation, which was evidently the result of the joint affection. Remembering my former case, I advised tapping, so as to ascertain the exact nature of the fluid, and if it was, as I suspected, sero-purulent, I further advised a free antiseptic incision. The result of the tapping determined that the fluid was turbid serum, and the microscope showed numerous pus cells in it. On the 21st the knee was freely incised, Drs Chiene and Ritchie assisting, and Dr Stewart of Chirnside, the patient's uncle, being present. Several ounces of the sero-purulent fluid escaped, and a most searching examination of the articular surfaces was made with the finger. The articular surfaces were perfectly healthy. A tube was introduced, and the wound dressed as in the former case. The tube was retained for four days and then removed, the wound being dressed carefully daily. Three days after, the patient left his bed for the first time, and in a month after he was walking about freely, the joint being supported by lateral pasteboard splints.

Not the slightest local or constitutional irritation followed the operation in either of these cases, but on the contrary a marked relief to the symptoms was the immediate result of the operation; and the complete recovery in both cases is most encouraging. My belief is that the operation in both instances prevented the acute suppuration of the articulation being completed, and therefore prevented the serious risks of this condition.

Fourth. In injuries of a doubtful nature.

Cases of dislocation or fractures near joints are occasionally very difficult to diagnose, and if such diagnosis be important in connexion with the treatment and satisfactory result of the case, I would not hesitate to make an exploratory incision, with the aid of antiseptics, provided other methods of examination failed to give the proper and full information.

Sharp fragments of bone, which may be perilling important bloodvessels or other structures, in cases of simple fracture, may be examined, and, if thought right, removed by such an incision; and I have reported a case of this kind in which I successfully extracted a sharp and separated portion of a fractured clavicle, lying over and endangering the subclavian artery.

Fifth. In obscure affections of the internal viscera, which admit of being relieved by surgical means.

Cases of intestinal obstruction, calculus of the kidney, and tumours or swellings in connexion with the abdominal or pelvic viscera, or conditions simulating these various affections, frequently give rise to dangerous symptoms, and if such conditions have failed to be relieved by ordinary carefully-applied methods of treatment, it seems to me perfectly justifiable that surgery should be resorted to, if the condition is one in which there is even a chance of its being relieved or cured by surgical assistance.

Owing, however, to the difficulty of correct diagnosis in many of these cases, the surgeon hesitates to interfere, and it is in such cases that I would advocate an antiseptic incision, in order to determine if and what operative interference can be of service.

A note of the following case is interesting in connexion with this subject:—

J. S., æt. 36, was transferred to my wards from the medical wards on the 27th of May last. This patient had been carefully treated by Dr G. Balfour on account of well-marked symptoms of renal calculus, which had existed for two years, and which were so severe as to prevent her attending to her duties. Medical treatment having failed to relieve her symptoms, it was hoped that surgery might do something for her. As her symptoms pointed so distinctly to an affection of the left kidney, I suggested an antiseptic exploratory incision—a proceeding which I recommended in a paper read before the Society five years ago.[1] The patient willingly consenting, I made a free incision in the left lumbar region, exposed the entire kidney and upper portion of the ureter behind the peritoneum, and made a most careful examination of the organ. The result of our examination was that, as far as could be ascertained, the kidney and ureter were perfectly healthy, and no trace of a calculus could be found. The wound was therefore closed and dressed in the usual way. No bad results followed the operation, but on the other hand the patient obtained complete relief from her symptoms, and she left the hospital cured, and thoroughly believing in the efficacy of the operation.

The application of the principle of exploratory antiseptic incisions is by no means limited to the conditions enumerated; but I trust that I have written enough to direct attention to what I consider to be a most important addition and assistance to surgical diagnosis and treatment.

To Mr Lister is due the credit and honour of giving to practical surgery successful and scientific antiseptics; and to him, therefore, I am indebted for what is truly the foundation of my suggestions in this paper.

ARTICLE V.—*Edinburgh Asylum Papers.* No. VI. *Cases illustrating the effect of Peripheral Irritations in Epilepsy.* By JAMES MACLAREN, L.R.C.S.E., Assistant Physician.

(*Read before a meeting of the Medico-Psychological Association, held in Edinburgh on 26th September* 1874.)

REGARDING the treatment of epilepsy, Schroeder Van Der Kolk wrote—"In no department of medicine does a more rude empiricism prevail. All kinds of secret and open remedies are tried without distinction, and a proper rational treatment, of which there is such great need, can scarcely be said to exist; a state of things dependent on total ignorance of the nature and essence of the disease, as well as of the causes which excite it, and of the peculiar morbid changes which keep it up. Everything rests upon loose conjectures, upon the temporary fame, now of this, now of another remedy; in fact, it has often appeared to me that many physicians, when called on to treat epilepsy, take refuge in a great box full of all kinds of medicines, among which those whereof they know not even the ingredients occupy no small place. Now, they close their eyes, grope blindly in the box, take out a remedy, and compel the unfortunate patient to venture the slight chance that, among the hundreds of remedies contained in it, this is exactly the one suited to his state and the nature of his illness. Meanwhile the first period, in which this disease, like insanity and other affections of the nervous system, may be cured, passes by unhappily and unused."

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[1] See Journal for July 1869.

To remedy this highly unsatisfactory state of matters, Van Der Kolk began those observations and researches which led him to the conclusion that the first cause of epilepsy consists in an exalted sensibility and excitability of the medulla oblongata, rendering the latter liable to discharge its nerve energy on the application of irritants which excite in it involuntary reflex movements, and which irritants may be of various kinds. To diminish this condition of the medulla, Van Der Kolk tried first ordinary narcotics, opium, morphia, belladonna, hyoscyamus, etc., and finding that these did not fulfil his object, had recourse to conia, strychnia, and digitalis, from none of which he obtained a satisfactory result. He then took to the use of external derivatives, whose use he advocates strongly, and which in his hands produced remarkable cures and intermissions of the disorder. Several cases are narrated by him of six or seven years' standing, in which, by the use of setons, issues, dry cupping, or some other counter-irritant, an apparently permanent cure was established, he of course endeavouring at the same time to remove or obviate any remote cause that might be giving rise to the epilepsy. He seems, however, to have applied the counter-irritation with little reference to the natural history of the special case he was treating.

In other hands the use of counter-irritations has been followed by those puzzling results which are so common in the treatment of disease. Reynolds says of it, "Counter-irritation has at some periods been largely employed with a view to the reduction of undue excitability; old records teem with details of its utility. Accidental burnings of the face in an epileptic have been followed by its cure. The actual cautery to the skull was highly recommended by Rudolph. Of late Dr Brown-Séquard has employed the cautery with good effect. It is, however, rather with the view of destroying the peripheric origin of an aura than for the sake of, by counter-irritation, reducing augmented excitability, that Dr Brown-Séquard has recommended its application. I have never ordered a seton or issue to be inserted, but I have frequently met with them still open in uncured epileptics, and have more frequently seen their scars in those who, deriving no advantage from them, had permitted them to heal over. Repeated blistering I have employed in epilepsy; also frequent sinapisms to the nucha; but I cannot say that these have been followed by any good result."

Echeverria narrates cases where the bromide of potassium having failed, counter-irritation applied empirically, and in one case accidentally, led to a cure of the disease. Brown-Séquard, regarding the treatment of epilepsy, after pointing out the necessity of ascertaining the peripheric or other origin of the disease, says, "The best means of treating epilepsy seems to consist in the application of a series of moxas along the spine, and particularly the nape of the neck."

Sir Thomas Watson says, "There is no *primâ facie* objection to counter-irritation, to the use of the croton-oil liniment, of blisters, of a seton in the neck; but I must confess to you that often as this expedient is employed, I have seldom witnessed any such result from it as would encourage me to expect benefit from repeating it in another." He quotes a most interesting case from Quain's edition of "Martinet's Pathology" of an epileptic boy who fell over a cliff, injuring his head, and who had no fits during the process of the healing of the wound. Dr Watson has since seen good effects from the insertion of a seton in the neck, but on the other hand has met with repeated disappointments to his hopes of a cure from this agent.

On the whole, balancing the opinion of those who have had experience and written on the subject, it seems very equally divided between those who have found counter-irritation an infallible remedy and those who have discovered little or no benefit in its use—a state of matters not altogether unknown in the treatment of other diseases. Latterly this treatment seems to have been superseded by an indiscriminate use of the bromide of potassium, the most directly curative agent for epilepsy that has yet been found.

I have not the excuse of having either a new theory to advance, or an elaborate set of experiments to describe, for troubling this meeting, for since giving notice of my paper I have not had time either to invent the one or conduct the other; but I have had recently under my notice what seems to me a very illustrative and typical instance of the benefit to be derived from a counter-irritation, and that too in a case whose origin is probably traumatic, and which would not be likely to be influenced by such a remedy.

X. Y., aged 40, was about twenty years ago thrown from his carriage in India, alighting on the left side of his head. His recollection of the accident, and what happened to him subsequently, is not very clear, but ever since he has suffered from pain in the side of the head and face, and about four years after the accident he became subject to severe epileptic fits, occurring at intervals of a month or six weeks, and followed by paroxysms of acute mania. In consequence of the direct history of the injury to the head, and the possibility of affording relief by means of surgical interference, Mr Syme performed the operation of trepanning over the seat of the pain, and removed a piece of bone, the only account of which given in the case-book is that it was of a suspicious nature. The wound healed well, and during the process the epilepsy was in abeyance, but soon after the fits recurred with severity, followed by the mania.

Brown-Séquard mentions a somewhat similar case, as regards the operationon the head, in which a lady who received a blow on the head became epileptic. A portion of diseased bone was removed by the trephine; but the fits continued after this was done. However, in the course of healing, the wound took an erysipelatous action. The erysipelas proved severe (or, in other words, there was great peripheral irritation), but she recovered from it, and never had a fit afterwards.

X. Y. was, after some years, put on half-drachm doses of the bromide of potassium thrice a day, which in his case was in some respects a failure, that is to say, it now and then diminished the number of fits, but did not lead to a cure. For some time the beneficial effects of the drug were very marked, the intervals between the seizures being sometimes lengthened to a space of 100 days, whereas previously he had a fit or a number together about once a month. However, it seemed ultimately to lose its effect on the malady, and was also apt to be attended with this result, namely, that it led apparently to a storing up of explosive force, so that if the seizures were at longer intervals they were more severe when they did come on.

Gradually his disease resumed much the same course as before the operation and the use of the bromide. He continued for some years to take fits, generally several in succession once in a month or six weeks. If a longer time passed, the epilepsyand subsequent mania were more severe. In September 1872, there being no one near, he fell in a fit against the grate, severely burning his left hip. In time a slough separated, leaving a very large circular wound at least seven inches in diameter, which continued open for thirteen months, till it was at last healed by skin-grafting. Now the interesting point of his case is, that through the whole of these thirteen months, during which the wound remained open, he never had an epileptic fit or an attack of mania.

When the wound healed the fits gradually returned, and are now about as frequent as formerly, and recently he had threatenings of a manaical attack, which was averted by sedatives. There is nothing special in the nature of the fits except their severity. He is often very stupid and confused for about half an hour before one, and during the convulsions sometimes bites his tongue. He has no ascertainable aura. In his mental state he displays during his mania much of the exalted religious excitement characteristic of the epileptic, and which has been fully described by Dr Howden of Montrose.

As in even the most apparently commonplace cases there are many points of interest that care and study will bring out, so there are several items in X. Y.'s history which deserve a passing notice. Space will only allow of one or two of the more prominent being remarked on. First, as to the cause of the attack. There is a strong neurotic taint in his family history: his mother was epileptic, and some more distant relatives were insane. He thus inherited, not, as the progress of his case has shown, insanity, but a predisposition in his nerve cells and bloodvessels to take on a diseased action; the insanity being only an outcome of this diseased action. With this easily affected brain, then, he devoted himself to a life of worry and fatigue and much mental effort, involving a wear and tear of, and strain on, his nervous system, which it was ill fitted to bear. Thus his brain was, as it were, ripe for explosion, and most probably possessed little or no recuperative power to enable it to recover from the effects of the mischief caused by the fall on the head, which by injuring the organ became the exciting cause of the malady. The mania seems purely a result of the epilepsy, and that is what I mean by saying that the *insanity* is not hereditary, but the neurosis is. Till the fits came on he was a man of more than ordinary capacity, and is so still during the remissions of his malady.

It seems at first sight remarkable that the severe operation on the head should not—acting merely as a counter-irritant, and applied early in the course of the disease—have had more effect on the epilepsy, seeing the effect produced later on by the superficial wound of the hip. The explanation probably is that, though apparently a very severe operation, its effect as regards shock to the nervous system is only to be measured by the extent of the wound to the periphery, which would not be extensive.

It is almost useless to speculate on the pathology of this case, and what are the exact intercranial changes which have taken place. Whatever their nature, they must, from the history and progress of the case, be deep-seated and important; but most remarkable of all the features of the case is the effect on these changes produced by the injury. The bromide, acting as a calmative and sedative, led to amelioration of the disease, and the superficial wound produced a similar or rather better result. No two remedies would at first sight seem less likely to bring about the same end, and yet, unlikely as it appears in theory, regarding the fact there can be no doubt. How the bromide acted, one can conjecture. It is not so easy exactly to do the same regarding the injury. Most probably, it seems to me, it acted not as a direct counter-irritant to any one locality in the brain or part of the nervous system, nor as a mere vascular derivative, but through the medium of shock. Of course, such an injury inflicted suddenly and continuing long must have caused a very great shock to the nervous system, and during its course of healing entailed a very considerable strain in the same direction. Consequently there would be a greater demand for nervous force, and less would thus be left to expend itself in those explosive efforts which eminent observers consider to determine the epileptic fit. That is to say, the wound, partly through the shock it produced and partly from the strain of its long continuance, became a derivative of nerve force.

Into the surgical question of the skin-grafting, which operation was performed by Dr Joseph Bell, it is beyond my province to enter further than to say, that this case affords a good example of the benefit that may be expected from this procedure in cases intractable to other treatment.

Two other cases of a similar kind I will just mention without dilating on. J. M. was admitted into the Royal Edinburgh Asylum in 1863. No history could be got, as he was extremely reticent, and there was no known relative. He turned out to be a confirmed epileptic, taking fits about twice a month. Early in 1873 he had his penis amputated owing to the development of cancerous disease in it, but neither the operation nor the subsequent healing process had any effect on the epilepsy. However, early in 1874, some enlarged glands in his groin ulcerated, producing large open malignant sores. He died in the beginning of last October of exhaustion from hæmorrhage; and from the time that these sores commenced till his death he had only one epileptic seizure, and that very early in the year, and before the wounds had reached any size. In his case, too, as in X. Y.'s, it was interesting to see the entire dependence of the mania on the epilepsy. J. M., while he took the fits, was frequently a dangerous and almost always an unmanageable troublesome man. For the eight months during which the epilepsy was in abeyance his mental condition was one of fair health, and he remained quite free from excitement.

A man died some little time ago, while under my care, who had been long in the Asylum, and whose attacks of epilepsy were almost daily and severe. On one occasion he fractured his humerus, and during the six weeks or two months that it took to unite he never had a fit. I have not minute details of this case, but I can vouch as to the fact mentioned. Considering that he was taking fits regularly before the fracture, and that they recommenced as soon as it was healed, it seems impossible to doubt that the injury to the bone had a direct effect on the disease, though how the throwing out of provisional callus would affect the medulla oblongata is difficult to imagine, unless it acted as the injury did in X. Y.'s case, by shock and strain on the nervous system, thus becoming a derivative of nervous force.

In each of the three cases it will be seen that the counter-irritation was applied in none of the localities which hitherto have enjoyed a monopoly of its use, namely, the crown of the head, the nape of the neck, or the course of the spine; and it is noteworthy that no aura or known source of irritation existed in the parts injured.

ARTICLE VI.—*Notes of a Case of Abscess of the Liver.* By GRAHAM STEELL, M.B.

(*Communicated by Dr Muirhead to the Medico-Chirurgical Society, 11th Nov.*)

P. M., æt. 53, an itinerant saw-sharpener, presented himself as a patient at the Stirling Infirmary, 10th September 1874, complaining of severe diarrhœa, with tenesmus and acute pain in the right hypochondriac region, these symptoms having existed for a week.

His whole appearance indicated that he was the victim of more serious disease than his complaints would lead one at first to expect. His countenance was anxious, and that of a person in great distress and suffering; his conjunctivæ and skin were distinctly but not deeply jaundiced; his breathing was very rapid and catching; his pulse accelerated, and his skin hot.

Such being his condition, he was at once admitted to the Infirmary as an in-patient. After removal to bed the following observations were made:—Decubitus dorsal, breathing very rapid (50 per minute), shallow, and thoracic, each inspiration seemingly causing him much pain. Pulse 108; temperature 103°; tongue thickly coated with a dense yellowish-white fur, through which some abnormally red papillæ projected at the anterior part; jaundice distinct. Abdomen not distended, but walls very tense, the recti muscles especially.

Great pain and tenderness in the right hypochondriac and in the epigastric region, but at no other part. On percussion, the hepatic dulness is considerably increased in the mammillary line. The lower margin of dulness extends fully an inch below the costal margin in that line; and on tracing it to the left it is found to pass into the epigastric region, nearly as low as the junction of that region with the umbilical, so that about three-fourths of the epigastric region is occupied by liver dulness. The tonicity of the abdominal muscles interferes somewhat with the palpation of the surface of the liver, but no unevenness or projection can be detected; on the contrary, the surface feels quite smooth.

On placing the stethoscope *over the portion of liver uncovered by the ribs, a distinct to-and-fro friction sound is audible* with the respiratory movements. Stools frequent, accompanied by tenesmus, yellowish brown or orange-coloured, and of very offensive odour. Urine contains bile-pigment, but no albumen. The heart and lungs present nothing abnormal.

Before proceeding to the narration of the progress of the case, it may be well to consider the above data, as to their collective diagnostic significance. They may be enumerated as follows:—

1. A week's illness. 2. High febrile movement. 3. Evidence of perihepatitis. 4. Jaundice. 5. Enlargement of the liver.

From these the following may be deduced:—

That the patient laboured under an acute affection, probably inflammatory in its nature, as shown by febrile movement and local pain. That the liver was the probable seat of such inflammatory action, as evinced by the presence of jaundice, and right hypochondriac pain. There was evidence of perihepatitis, not only in the pain and tenderness on pressure over the liver, but in the double friction sound audible on auscultation, and which could be produced only by the exudation of lymph upon the surface of the liver. That the parenchyma of the liver was also involved in inflammatory action, and the probable starting point of it, as deduced from the enlargement of the organ, from the presence of jaundice without any obstruction to the flow of bile into the intestines, from the severity of the fever, and perhaps also from the diarrhœa, the cause of which probably was an alteration in the composition of the bile, whereby it became irritating to the intestinal mucous membrane. As to the kind of inflammation, it was acute, and probably "hepatitis vera circumscripta, suppuratoria."

The patient admitted that he had habitually indulged in alcohol to great excess, and it might be said that such habits gave strong probability to the view that the disease was only acute congestion of the liver, previously in that state of chronic inflammation which commonly terminates in cirrhosis. That there was more than mere congestion of the liver was proved by the existence of perihepatitis. Frerichs, in his work upon diseases of the liver, mentions, however, in his records of autopsies of cases of cirrhosis of the liver, several cases where adhesions to neighbouring parts were formed, doubtless the result of a previous perihepatitis. But in such cases the adhesions probably take place gradually, and under very different circumstances from those under which the evidence of lymph upon the surface of the liver was found in this case. The perihepatitis occurring in cirrhosis is perhaps analogous to the pleurisy originating the thickening and adhesion of the pleura in cases of cirrhosis of the lung or fibroid phthisis. In such cases the inflammatory action giving rise to these changes is seldom recognised by the physician, as patients commonly do not seek advice until the pleuræ are thickened and adherent.

The height of the fever has been already mentioned as an argument in favour of the existence of hepatitis, and in relation to this subject the non-occurrence of rigors may be remarked. However, in the presence of thermometric proof of fever, this subjective phenomenon, depending upon a sudden alteration in the relation between the temperature of the body and that of the surrounding atmosphere, is unimportant. The absence of irregular and repeated rigors, indicating the occurrence of rapid elevations of temperature, would oppose the diagnosis of a series of metastatic deposits in the liver.

As to there having been a hydatid in the liver which had suppurated, the diagnosis of abscess would still be justified.

Progress of the Case.—The patient got gradually worse, the jaundice deepened, the diarrhœa persisted, notwithstanding the use of various astringents and moderate use of opium; the pain abated somewhat, and the friction ceased to be audible, except at one or two small spots. Fever continued of a remittent type—evening temperature 103°, morning 102°, but latterly declining in height as the patient began to sink. His appetite was entirely gone, and it was with difficulty that he could be got to take food at all. About a week after admission a distinct bulging began to show itself in the right hypochondriac and the epigastric region, extending from the right costal arch to somewhat beyond the middle line. This gradually increased, and indistinct fluctuation became perceptible over it. On inserting the needle of an aspirator into this projection, three or four ounces of thick pus were withdrawn. This placed the diagnosis of abscess beyond doubt, but the patient sank exhausted, and died on the 22d September. He did not seem to suffer in any way from the puncture, and it is only to be regretted that evacuation of the abscess was not attempted at a much earlier period.

No post-mortem could be obtained, so that the etiology of the abscess must remain undetermined.

The only feature of interest, in the absence of a post-mortem and a microscopical examination of the evacuated pus, which this case presents, is the aid afforded by the stethoscope to the diagnosis of hepatic abscess at an early period. Hepatic friction seems to be quite disregarded by the majority of authors on diseases of the liver; no doubt, because of the fact that when the condition which can alone give rise to it is present, there are symptoms, such as severe pain and tenderness, which render this sign superfluous. But as so-called pleuritic stitch is not invariably a sure symptom of the existence of pleurisy, so also pain and tenderness may exist over the liver without an actual perihepatitis; while friction over the liver is as sure a sign of perihepatitis as over the lung it is of pleurisy.

As to the possibility of an irregular surface of the liver and the "unequal pressure of tumours" giving rise to a friction sound, the authority of Skoda is opposed to such being the case in regard to the pleura, unless the surfaces of the projections are roughened.

Part Second.

REVIEWS.

Outlines of the Science and Practice of Medicine. By WILLIAM AITKEN, M.D., F.R.S. London: Charles Griffin and Co.: 1874. Pp. 593.

WHATEVER the title may be, the size and the authorship of this work conclusively prove it to be an attempt at a grinding book, a *multum in parvo*, which is intended by salient hints to recall to the mind of a well-informed and well-instructed student the many practical points which it is of importance for him to remember, and which his own thorough knowledge can easily amplify. In the preface, the object of the book is stated to be to supply "what is solid, practical, and essential for the student to learn in the Science and Practice of Medicine at the outset of his career." These two objects are pretty much the same—the former is the more practical, and probably the actual *raison d'être* of the book; the latter is very much a euphemism. In the one case the hints supplied must, to be efficient, be supplemented by reason and memory, in the other by teaching and reading.

In these days, when so much is expected of the student, we must of necessity be exacting with the teacher, and unhesitatingly condemn when all is not fairly up to the mark; and if in any case our critical judgment should be more unsparingly exerted than in another, it ought certainly to be so in the case of a Professor in the Army Medical School—a school which sets itself above all others by assuming the right to examine and reject, if in its opinion unqualified, the licentiates of all other schools, not only in some branches peculiar to itself, but in all the branches of medical science.

Opening the book, then, by chance, we commence with the subject "General Dropsy," which is said to be "an accumulation of serum in the areolar tissue, with or without effusion into the serous cavities." And here at once we meet with an absence of strict accuracy, derogatory to the value of the book and to the position of its author. General dropsy is anasarca solely, or it is anasarca *plus* something else. Very possibly a student might not be rejected for giving such an answer, but we can tell Dr Aitken that such a reply would lead to a very searching examination into the strict accuracy of his knowledge on other points. It is no use calling this hypercriticism; laxity of criticism leads to laxity of teaching, and the punishment for that falls on none more heavily than on the poor student, and nowhere more frequently than at Netley.

Take, again, his remarks on differentiating the pulmonary and aortic second sound. He says, at p. 425, that we must listen at the "second right and second left costal cartilages for the sound of aortic and pulmonary orifices respectively." But the second left costal cartilage is quite above the pulmonary artery; and anatomy and acoustics both agree in teaching us that the third left and second right costal cartilages are those points at which we may most easily differentiate the pulmonary and aortic sounds from one another. On the same page there is also shown a want of due acquaintance with the relation of the pulse to aortic regurgitation, in which retardation is only said to be "frequent." The chapters on Nervous Diseases are equally defective; there is no hint as to the very frequent connexion between chorea and rheumatism; and paraplegia is divided only into two forms, reflex paraplegia, and paraplegia depending upon myelitis. It is useless going over the whole book *seriatim;* similar defects are to be found on every page, even in respect to the diagnosis and pathology of disease, while all the paragraphs on treatment are mere catalogues of those remedies which have been employed in the diseases referred to, without even an attempt at indicating any definite reason for preferring one remedy to another in any special case. But, indeed, what trust can any one repose in a teacher of therapeutics who says, "There can be no doubt that we have it in our power materially to modify the course and to shorten the duration of pneumonia by the judicious employment of blood-letting, leeches, tartar emetic, certain salines (not specified), and opium"? Heaven help the poor patient against whom so many big guns were fired! it would be a very bad marksman indeed who would not with these effectually cure him, not only of his pneumonia, but of all present or prospective ills. Or, what are we to think of one who, in this present year of grace, says, in reference to delirium tremens, "The disease must be treated as one spontaneously curable; not by withholding remedies, but by using them in strict subordination to good nursing, in a darkened room, with carefully-adjusted diet and regimen"? The chief remedial agents recommended being cayenne pepper and soup—not a word about the use of bromide of potassium, and the very name of hydrate of chloral never once mentioned. If this work is to be taken as a specimen of the kind of knowledge required at Netley to pass in Practice of Medicine, we cannot wonder at the failures hitherto recorded, and we recommend those of our readers who think of going thither, to purchase this work, as we do not know how else they can nowadays possess themselves of the same kind and quality of information.

Notes on Surgical Nursing: Being a short Course of Lectures delivered at the Training School for Nurses in connexion with the Liverpool Workhouse. By J. H. BARNES, Surgeon to the Workhouse Hospital. London: J. and A. Churchill.

THE Liverpool Workhouse Hospital was the place where, under the direction of the late Miss Agnes Jones, unpaid paupers were replaced by skilled paid nurses, with immense advantage to the sick.

Mr Barnes has written a set of lectures with no pretensions to originality or even completeness, but still containing a large amount of valuable instruction conveyed in a simple direct manner.

Nursing is now, especially in hospitals, a very different thing from what it used to be. Instead of the traditional Mrs Gamp—old, often snuffy, always coarse, occasionally drunken—in all well-regulated hospitals we have an organized staff of trained and educated ladies, with *esprit de corps*, kindness, and grace. We are glad that our Edinburgh Infirmary is already in the van of progress in this good work.

Aus der Gebäranstalt, etc. Klinischer Bericht für die Jahre 1840–1871. Verfasst von Dr **Ernst Bidder** und Dr **Wassily Sutugin**. St Petersburg: 1874.

The Lying-in Institution, etc. Clinical Report for the years 1840 *to* 1871. Prepared by Dr **Ernest Bidder** and Dr **Wassily Sutugin**. St Petersburg: 1874.

This is an excellent specimen of the hospital reports which are now so well known, and which have done so much service to medical science. That of Collins of Dublin was a model, suited for its time, a work of genius, and has had many imitations. Such a work must adapt itself to its day, and be, to a great extent, a review, by the light of the new hospital data, of the current and novel statements and theories. Authors of such reports have plenty of scope for showing how extensive and deep is their knowledge of the progress of midwifery, and in this respect Drs Bidder and Sutugin are a good example.

Obstetricians will find here many valuable data and criticisms; and it is not they only who may receive instruction. We would recommend to Mr Erichsen's notice the evidence afforded by the table of the number of births and mortality per cent. He will find it to be not at all in accordance with the tenor of his views on the subject of hospitals, and the influence of their size. The same evidence is given by the statistics of the Dublin, Paris, and Vienna hospitals, and should be convincing so far as its influence justly extends.

Many British obstetricians had the pleasure recently of becoming personally acquainted with Dr Sutugin, and will now be glad to see him worthily represented in obstetric literature.

Die Fortschritte der Kriegsheilkunde besonders im Gibiete der Infections-Krankheiten. Rede gehalten von **Rudolf Virchow**. Berlin: 1874.

After remarking that the separation of practitioners into two classes, viz., surgeons and physicians, which prevailed in the middle ages, and still to a certain extent in England, no longer holds in Germany, where medicine is studied as a whole, the author, in this address, delivered at the celebration of the eightieth anniversary of the Military Medical Institution of Berlin, discusses the nature of the diseases to which armies in the field are liable. These diseases he divides into two great groups, viz., the native and exotic. To the latter class belong the plague, smallpox, and cholera to the former, typhus fever, dysentery, diphtheria, surgical fever, etc. He shows that all these diseases are accompanied by the development of bacteria and micrococci in the tissues and in the blood, and endeavours to prove that they are produced by the bacteria and micrococci secreting during their growth a poisonous material, which has a specific action on the system.

As showing the great efficiency of the Prussian Army Medical Department, it may be mentioned that while in the Crimean war the French lost one man in three—(95,615 men it is calculated having lost their lives, of whom 10,240 fell before the enemy, about the same number died in the hospital, and the remainder, or upwards of 75,000 men, were victims of pestilential diseases)—in the recent Franco-German war, out of a total of 913,967 men, the entire loss was 44,890. Of these, 17,572 fell on the battlefield, 10,710 died in hospital of their wounds, and the remainder, 12,253, succumbed to fever and other diseases. These favourable results he ascribes to three causes: 1. To the experience in the two short previous wars; 2. To the invaluable reports of the American Army Medical Department; and, 3. To German science.

Théorie Physiologique de l'Hallucination. Par le Dr ANT. RITTI. Paris: 1874.

IN this pamphlet of seventy-five pages, the author exposes a theory of the origin of our hallucinations which has been advanced by Dr Luys. The work has that clearness of style and orderly arrangement often met with in French scientific writers. The theory is thus summed up by the author: "Hallucination is a morbid symptom, which has, as its anatomical substratum, the nerve-cells of the optic thalami, whose function it is to receive and elaborate outward impressions. These centres may, under the influence of different pathological causes, be put in action automatically. Impressions resulting from fictitious irritation of the cells follow the same course as impressions resulting from their normal activity, and radiate through the white fibres into the network of the cells of the gray matter of the brain. These latter, thus put into vibration, may produce an indefinite series of delirious conceptions, which have an appearance of system and originality in proportion to the amount of energy and vitality preserved by nerve-cells."

The automatic irritation of the nerve-cells of the anterior centre of the optic thalamus radiated into the intellectual cells of the brain, will produce hallucinations of smell; that of irritation of the cells of the "middle centre" produces hallucinations of vision. In like manner, hallucinations of the general sensibility result from irritation of the "median centre," and hallucinations of hearing from the "posterior centre;" false visceral sensations arise from irritation of the gray central substance. To distinguish different parts of an organ by such clumsy terms as middle and median centres—centre moyen et centre médian—and gray central substance, surely shows some poverty of language.

The author records a number of observations where hallucinations were found connected with disease of the optic thalami, almost always accompanied, however, by lesions in the hemispheres and other parts of the brain.

It strikes us that materialists, in their efforts to explain the wonderful operations of the human mind, by connecting them with real or assumed changes in the cells, molecules, and fibres of the brain, generally delude themselves with words which have no meaning. Dr Ritti talks of a nerve-cell vibrating. We can understand well enough how a cell can vibrate,—that is, it can move up and down very rapidly;—but when they insist that this is the reason why we perceive that four multiplied by three is twelve, we do not profess to be grateful for the explanation.

We can no more connect in our minds the production of an abstract idea with change of form, or place, or the addition or subtraction of atoms of matter within a cell-wall, any more than we can conceive of an idea being bounded by a substance.

The evidence connecting hallucinations with diseases of the optic thalami, is deserving of attention. The existence of false sensory impressions upon one side—as, for example, when visions are seen with one eye, and not with another—is very curious.

We have read Dr Ritti's pamphlet with interest, and beg leave to recommend it to those who pay attention to such inquiries.

Virchow's Archives, Bd. LXI. s. 289. "*Noch Einmal die Keratitis.*" Von J. COHNHEIM in Breslau.

NOTWITHSTANDING all the labour that has been expended in investigating the *genesis* of lymph or pus corpuscles, the subject still continues to excite much controversy, especially in Germany.

Since the "epoch-making" work of Cohnheim (uber Entzündung und Eiterung), published in 1867, it seems to be pretty generally admitted that pus corpuscles are in great part derived directly from the blood, and that in the inflamed spot they pass in some way through the vascular walls into the tissues. The exact manner in which they affect the transit is still undetermined. Cohnheim, in his original treatise, suggested that it was by means of their independent powers of locomotion; but more recently his views have been modified, and he now seems not unfavourably disposed towards the hypothesis of Hering and Schlarewsky, who regard the phenomenon as an example of the filtration of a colloid substance through the physical pores of the vascular walls. In his later work on the subject, entitled "Neue Untersuchungen über de

Entzündung," published last year, Cohnheim draws attention to a condition which was clearly pointed out by Lister in 1858 (*Phil. Trans.*), namely, the altered condition of the walls of the bloodvessels in an inflamed part. He says,—"Whatever may be the nature of the alteration of the vascular wall, whether in its totality it has become softer or more porous, or however it may otherwise differ from the normal condition, I venture no sort of guess. I prefer to limit myself to the general remark, that the vascular walls, owing to the alteration which they have sustained, have become unable to retain the blood within them, so that a blood pressure, quite inconsiderable, less even than the normal, suffices to press the contents, corpuscles and fluid as well, through the wall."[1] There is thus much room for difference of opinion regarding the manner of the transit, but as regards the fact of its occurrence there is scarcely any.

The chief point of dispute at present is, whether pus corpuscles are all derived directly from the blood? or whether a proportion of them may not be formed at the inflamed spot by the proliferation of the connective tissue corpuscles, in the manner originally propounded by Virchow? That such proliferation constitutes a second source of pus cells is strenuously maintained by Stricker and others, whereas it is denied *in toto* by Cohnheim and his disciples.

It cannot be disputed that Stricker and his followers find great difficulty in proving the affirmative. The tongue of the frog is one of the parts which can be easily subjected to microscopic examination in the live state, and it contains numerous well-developed connective tissue cells. Yet when inflammation is artificially excited in it, and the behaviour of its connective tissue cells is carefully watched, no very remarkable changes are observed within them. "Stricker," says Cohnheim, in the present article, "observed this object with the most praiseworthy endurance for ten hours consecutively. And the result? That he rose from his work-table without having observed a single cell division, or even a progressive constriction in any one of the connective tissue cells." Stricker, however, attributes this want of success to the comparative tardiness of the process of proliferation, and to the fact that, in the tongue, before there is time for its occurrence, all the elements of the tissue become obscured by the crowding of the migrated corpuscles within the area of inflammation. He has, therefore, in the meantime, abandoned the tongue, and betaken himself to the cornea, in the stellate connective tissue cells of which he believes that, in conjunction with his pupil W. Norris, he has distinctly observed the beginning of proliferation and formation of pus corpuscles (Studien aus dem Institut für Experiment. Path. in Wien aus dem Jahre 1869).

When the healthy cornea of a frog or rabbit is excised, and immersed for a time in a dilute solution of the chloride of gold, its stellate connective tissue corpuscles are stained with the gold, and present, with their ramifying processes, a beautiful object under the microscope. Now, if keratitis be artificially excited in a number of frogs at the same time, and the affected corneæ be, one by one, excised at intervals of hours or days, the changes produced by inflammatory action in the connective tissue corpuscles can, with the aid of the staining process, be observed as it were stage by stage. This is the manner in which the investigation is carried on by both the contending parties. The inflammation can be excited in various ways, sometimes directly by cauterizing with nitrate of silver or a hot wire, or by incising the cornea, or passing a thread through it; sometimes indirectly, by passing a thread through the globe of the eye, and thus exciting a panophthalmitis, in which the cornea may participate only after the other structures have been involved.

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[1] Neue Untersuchungen, s. 31.

The changes originally observed by Stricker and Norris will be found excellently figured and described by Dr Burdon Sanderson, in his article on inflammation (*Holmes's Surgery*, vol. v.). They are briefly, that within a few hours after inflammation has been excited, the corneal corpuscles are found to have drawn in their wonderful network of processes so that they are no longer stellate, but simply rounded in shape; that, in sixteen hours or so, they are found to be enlarged, and to be filled with numerous nuclei, so as to present in all respects the characters of large nucleated masses of protoplasm—the giant cells of Virchow or the *plâques à noyaux multiples* of Lebert; that further observation is obscured and rendered uncertain, by the crowding over the whole field of migrated corpuscles from the edge of the cornea.

According to Cohnheim, who described the phenomena of corneal inflammation in his original treatise, there is no cell formation in the corpuscles. On the contrary, he believes that after drawing in their processes in the manner described, they speedily show symptoms of degeneration, many vacuoles appearing in their substance, and their protoplasm, as a whole, becoming granular. When a corneal inflammation is *induced* by exciting panophthalmitis, he observes no change whatever in the corpuscles; they do not even draw in their processes, even should the whole field have become crowded over with the migrated cells. When he meets with the nucleated masses of protoplasm above referred to, he attributes them, not to the proliferation of the corneal corpuscles, but to the welding together into masses of a proportion of the wandering cells.

Cohnheim's views have lately received powerful support from the independent investigations of Professor Axel Key, and Med. Lic. C. Wallis of Stockholm (*Virchow's Archiv*, vol. lv.). These observers have examined the phenomena in several hundreds of frogs, the result being that in every important particular their observations coincide exactly with those of Cohnheim.

On Stricker's side of the question, the latest contribution is a paper by Professor Böttcher of Dorpat, published last year in *Virchow's Archiv*, vol. lviii., and to this the present paper of Cohnheim is a reply. Professor Böttcher's paper was already in the press when he received a copy of Cohnheim's "Neue Untersuchungen," and found that the views which he had been contesting had undergone considerable modification at the hands of their author. He nevertheless published the paper, as he thought the facts which it contained might be of service, and he added to it a postscript which has special reference to the more recent modifications of Cohnheim's views. In the paper, the author fixes as a salient point upon cases of keratitis, in which the inflammatory changes are solely confined to the irritated point and its immediate neighbourhood. There is in such cases no evidence to show that the cell deposits have come from the margin, since there is no marginal cloudiness of the cornea, nor any accompanying congestion of the conjunctival vessels. Such cases, as Professor Böttcher subsequently found, have been carefully described in the "Neue Untersuchungen," but they had been overlooked by Cohnheim in his original treatise. Böttcher thinks that in these cases the cells are evidently formed at the spot inflamed, as, on investigation, he finds around the injured spot the corneal corpuscles not only enlarged and containing many nuclei, but also constantly associated with numerous small particles of protoplasm, which he thinks have been thrown off by the cells, and may subsequently develop into pus corpuscles. He objects to the use of the nitrate of silver as a corrosive, since he believes that it destroys the vitality of the cells over a wide zone around the spot touched. The chloride of zinc, or the insertion of a fine thread into the thickness of the cornea, but not through it, he thinks preferable as means of irritation. In his postscript he declares his belief that the migration of the white corpuscles, as now explained by Cohnheim, differs little, if at all, from ordinary extravasation—an occurrence long familiar to observers as a common phenomenon in inflammation.

Cohnheim, in his reply, complains at the outset that his opponent had not carefully read his latest work. If he had done so, his paper would probably not have been written. In the course of the reply, Cohnheim asserts that no one of his opponents has been able to raise the slightest objection to his description of the phenomena of keratitis, when it is *induced* from a panophthalmitis, and yet he says if he were to employ the weapons of logic, he might say that "as there is here a legitimate and undoubted keratitis, in which, in spite of the presence of enormous numbers of pus corpuscles, the stellate cells remain unaltered, it follows that the change of form in such cells, when inflammation is otherwise excited, cannot be reckoned a characteristic and essential phenomenon of the process." But, he adds, he will rather meet his opponents upon their own grounds, and confine his remarks to the phenomena observed when the inflammation is directly excited by injury of the cornea itself.

In the first place, with regard to these cases of corneal inflammation, in which there is no evidence of migration from the margin, but only the presence of pus cells at the irritated spot in the centre, he remarks that such an appearance is never observed unless, owing to injury or subsequent sloughing, the irritated spot be deprived of its epithelial covering, and the tissue of the cornea be thus exposed to the entrance of fluids from the conjunctival surface. The natural conjunctival secretions contain, he says, a limited number of leucocytes, which is increased when the cornea is irritated; and such cells, passing into the cornea by the perforation, crowd often, in a radiating manner, in and around the wound, giving the spot an opaque cloudy appearance, whilst the remainder of the cornea may remain quite transparent.

After describing in detail the variations of the phenomena according to the severity of the inflammation, etc., he thus sums up his conclusions, page 309,—"That is, then, the history of circumscribed keratitis, as I have been taught it, by numerous systematic investigations, conducted both upon frogs and mammals at different seasons of the year, and according to the most varied methods. It is in reality simple enough. By some sort of violence, it may be corrosion, cauterization, or incision, the spot directly affected, and also an annular zone of greater or less width, are injured and their contained cells become indiscernible, or more or less altered in form and appearance. Among the injured structures of the spot affected, is the external epithelium, which in general is sooner or later thrown off. So long as the epithelium remains, there is scarcely any further change; but so soon as it is thrown off, and an opening is thus made, the conjunctival secretion, with its contained elements, passes into the exposed tissue of the cornea. The quantity of fluid thus admitted depends upon the size of the opening, and the time which elapses before it is finally closed up. In the case of a small opening, with very speedy regeneration of the epithelium, the quantity may be very unimportant, whilst in the opposite conditions it may be very considerable. The fluid passing into the tissue causes, as it mingles itself with the inter-fibrillar glue substance, a distension of the inter-fibrillar spaces (spear and needle figures). The pus corpuscles move forwards with especial preference into these inter-fibrillar spaces, and thus form long rows, often in single file. Not unfrequently they pass at once into the openings of the canals which lodge the corneal nerves, along which they extend beside the nerve fibres. Meanwhile the disorganization of the stellate corpuscles in the corroded area, and eventually in its immediate neighbourhood, is continually increased by the influx of the entering fluids. The greater the quantity of pus corpuscles, spear-figures, and oil-drops, the more difficult naturally becomes any sharp differentiation. All this comes to an end at the moment when the regeneration of the epithelium is completed. Then begins the gradual restitution which I have already attempted to describe."

Cohnheim, in this paper, as in his "Neue Untersuchungen," describes three possible effects of corrosives upon the cornea:—

1. When very slightly applied, a mere local trace of dimness, due to the direct action of the corrosive, which, if the epithelium is not thrown off, disappears in course of time, without any trace of pus cells, either at the spot or the margin of the cornea.
2. When more severely applied, the throwing off of the corroded epithelium, and the central accumulation of pus cells, as just described, the remainder of the cornea remaining quite normal.
3. When more severe still, the same central pus formation, and also a marginal invasion of the cornea with migrating cells from the vessels. The cloudiness from each of these sources, he says, can be seen extending over the cornea in opposite directions, until at last they meet, and the whole cornea is rendered opaque.

Throughout the paper, Cohnheim treats his opponents with scant ceremony. He complains of their loose way of using terms. He accuses them of talking vaguely of "young cells," when nobody knows how young or old a cell may be, and he objects to the use of such indefinite terms as "irritant" and "irritation," remarking that Prof. Böttcher, when he touches the cornea with chloride of zinc, and finds what he anticipated, speaks of the result as being due to "irritation;" but if the effect is not what he looked for, he calls it "death." Supposing, he says, he should grant (which he does not) that in some of these cases, a great many small nuclei may sometimes be found where formerly there was only a large one, what then? The big one has merely broken down into a number of little ones; and, as Fleming (*Virchow's Archiv*, vol. lii.) showed with regard to fat cells, such breaking up of the nucleus may be merely an indication of commencing degeneration. Böttcher's assertion, in his postscript, that the migration of the corpuscles, as Cohnheim now understands it, is nothing more or less than extravasation, Cohnheim treats with scorn, declining altogether to argue with an author who cannot comprehend the difference between the two phenomena. On the whole, it appears to him curious how pathological anatomy should have become such a tumbling ground (tummelplatz) for speculations of all sorts, and how the most steady-going, sensible investigators no sooner step within its area than they begin to make "the most foolhardy leaps into the regions of hypothesis." He recommends that people in this, as in every other science, should prove, and not merely assert. He concludes the paper with a parting thrust at another opponent, who formerly differed much from him, but in a later treatise has yielded several points. Wishing to see how the remaining difference was maintained, Cohnheim carefully read his paper (*Der Entzündungsprozess*, von Samuel, Leipzig, 1873), and was proud to discover that the author had at different places appropriated word for word whole sections out of his works on the subject, and had employed them for his own arguments. He hopes the honoured author will, on further study of his last work, give up the few remaining points of difference, and be convinced of the truth of his description. If the author has not thought it necessary to give him one word of thanks for having helped him into the right way, that will not prevent him from expressing his lively gratification at having won so valiant a comrade in arms to fight for his own doctrine.

We shall probably soon hear more from the other side of the question; and, on the whole, it is not likely that there will be any temptation to sacrifice the interests of truth to too sensitive a regard for the delicacies of criticism.

Meanwhile, we may ask ourselves the question—Has any solid corn come out of all the threshing which the subject has been subjected to within late years? Unquestionably there is one fact of immense importance ascertained, we may surely say for certain, namely, that in most forms of inflammation great numbers of pus or lymph cells pass directly from the vessels into the tissues. It cannot be said definitely in what way they get through the wall, it may be by "filtration," or "transudation," or "migration;" that is still an open question. However it may be explained, it is scarcely possible to doubt the fact, when one examines the beautiful demonstration of it in the frog's mesentery, and observes the crowding of the corpuscles on both sides of the vascular wall, outside and inside. The conviction forces itself upon the observer, that in some way the corpuscles pass through the wall, even should he not be so fortunate as to detect individual corpuscles in the act of passing.

As regards the participation of the connective tissue corpuscles, we must, it is to be feared, still submit to remain in doubt. Results of experimentation upon the cornea have been contradictory and confusing. After Cohnheim, in his first work, had asserted that, in all forms of keratitis, the pus corpuscles came from the margin, it must have been rather staggering to him. to have discovered that in many cases the pus cells exist exclusively at the irritated spot and its neighbourhood. It seems at first but a lame explanation, that in these cases there is always a penetration through the epithelial coat, and that the conjunctival fluid enters through this into the tissues, and deposits pus cells. On examination, however, the explanation is not so lame as it looks. It derives especial support from an observation incidentally recorded by Böttcher, and referred to by Cohnheim, namely, that in some cases in which the centre of the cornea had been slightly cauterized, there was no change for some days but the primary dimness produced by the escharotic; but that, immediately upon tearing up the centre of the spot with a needle, a little crowd of pus cells began to collect itself in the wound, and to spread itself into the vicinity. It is to be hoped that physiologists investigating this question will soon find some way of protecting the cornea from the eyelid, and otherwise so guarding it that there will be no possibility of either conjunctival secretion on the one hand, or aqueous humour on the other, coming in contact with the wounded surface. We should then, perhaps, be able to ascertain with more exactitude what cell-producing power the corneal tissue itself can display when under the action of inflammation.

Lastly, it may be said, that should all efforts to demonstrate cell-formation in the cornea fail, the physiologist can still have recourse to cartilage, in which cell division, under the influence of inflammatory irritation, has been a familiar fact since the demonstrations of Goodsir and Redfern.

The Hand. By Sir CHARLES BELL, K.G.H., etc., etc. Ninth Edition. London: George Bell and Sons: 1874. Pp. 428.

IT may seem superfluous to notice in these pages the ninth edition of a work once so well known and so much admired as Bell on the Hand; it is, however, forty-two years since this book made its appearance, and the generation who knew and loved the author, and who were charmed with the beauty of his diction and the philosophy of his sentiments, have chiefly passed to the majority, and are replaced by another, to whom both the work, and especially its philosophy, seem as antediluvian and as strange as a fragment of an ichthyosaurus. For this reason, we desire to call attention to this new and carefully-edited edition of this work, which must always be classical while the English language continues to be spoken; and we do not shrink from avowing our belief in its philosophy. This work was originally published as one of the Bridgewater Treatises, so called because the Right Honourable and Reverend Henry, Earl of Bridgewater, left a sum of money in trust, among others, to the President of the Royal Society for the time being, to be paid to the authors of a series of works illustrative of the power, wisdom, and goodness of God, as manifested in the Creation. The task which, in accordance with the terms of this bequest, was given to Sir Charles Bell, was to illustrate, with reference to the Hand of Man, a wise, intelligent, and beneficent adaptation of the organization and instruments of animals, on the one hand; and the elements in which they are to live, the position which they are to hold, and their means of obtaining food, on the other—both with respect to animals which have existed, and those which do exist at the present time; an argument which, in its essence, and in its mode of illustration, is wide as the poles apart from that prevalent in the present day; and yet one which, customary as it now is to sneer at teleological views, is based, we hold, on a sounder foundation than that which has mainly replaced it, and one which will assuredly regain its importance when the eyes of philosophers have ceased to be attracted by the brilliance of the

Darwinian hypothesis, which, like the Will-o'-the-wisp, allures but to mislead. For what is this hypothesis? According to its most distinguished expositors, it does not attempt to explain the origin or creation of life; it merely endeavours to prove that life having once been created in the simplest forms of organization, these were subsequently left to find their way to higher levels, in accordance with certain so-called laws, which have been variously designated as "the survival of the fittest," "natural selection," and "sexual selection." That is, the higher forms of animal life have either been self-created, or they are the survivors of a few out of many accidental modifications of pre-existing organisms. Probably the exponents of this doctrine may not accept as a true statement of their views the actual fact that, given a mass of protoplasm, their hypothesis is based upon chance, the results being modified by natural and sexual selection. Natural selection has signally failed to account for all the phenomena involved, and sexual selection has been adopted to supplement it, but no indication has ever been given of how the sexes came to be differentiated. What rational probability is there that a fissiparous germ should develop its dividing portions into male and female? Any other modification would be worse than a continuous fissiparous generation, it would be the cessation of development in that line. What, again, is the probability of two neighbouring germs developing, the one two females, the other two males; and what is the probability in favour of these accidental modifications continuing the species? According to the doctrine of chances, the probability of any such occurrence must be inconceivably infinitesimal. And the existence of sex, and all the weal and woe that depends upon it, is a hitherto untouched argument in favour of teleology—wisdom, beneficence, and design in creation.

Apart from this argument, however, and granting the possibility of all that Darwinians desire, the conditions of our globe are unfitted for their development; thirty millions of years are postulated to produce only some of the important modifications now existing; but according to physicists, reasoning from acknowledged data, it is not more than thirteen millions of years since our globe was a mass of molten matter, upon which no life could exist (Sir William Thomson). The book of Genesis, that wonderful post-historic apocalypse, contains, for obvious reasons, no strictly scientific account of creation; but while it inculcates unalterable religious and moral views, it also contains nothing inconsistent with anything as yet proved to be true in science; and that is more than can be said of any other work of equal age and similar pretensions, and very much more than, as we have seen, can be said of the latest phase of materialism, the Darwinian hypothesis.

We have much pleasure in recommending, especially to our younger readers, this most instructive and interesting example of teleological argument—Bell on the Hand; and they may perhaps peruse it with additional interest when they learn that the preparation of this edition has been the result of the devoted affection of Sir Charles Bell's now aged widow. An admirable account, by Alexander Shaw, of Sir Charles's Discoveries in the Nervous System is prefixed to the volume, and this, if possible, enhances its value.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING II.

Wednesday, *2d Dec.* 1874.—Dr D. R. HALDANE, *President, in the Chair.*

I. *Mr Annandale* showed—(1.) A BOY, æt. 8, who was suffering from a DOUBLE HYDROCELE communicating with the abdominal cavity. On tapping the left tunica vaginalis 18 ounces of serous fluid were drawn off, with the result of relieving the hydrocele upon both sides, and also an abdominal swelling which had been observed. Three weeks after the final tapping, 12 ounces of fluid were again drawn off from the left tunica vaginalis. The fluid had again accumulated to as great an extent as ever.

(2.) A girl, æt. 14, whose HIP he had excised three months before. The wound was soundly healed, and the movements of the new joint were free and extensive.

(3.) *Mr Annandale* then showed SPECIMEN OF EPITHELIOMA, implicating the lower lip and jaw. He remarked that he wished to direct attention to it for the purpose of pointing out what he believed to be a useful variation in operating for this disease.

In cases where the epithelioma implicated the lower jaw, the removal of the affected part in its whole thickness seriously interfered with the patient's after powers of mastication. To obviate this, he had in the case brought forward first made a free incision through the soft parts, cutting wide of the disease. By then sawing through the jaw at the symphysis, he was able to ascertain how far the jaw was implicated, and then, as he found it did not extend through the whole thickness, to saw off the outer table only. By means of holes drilled, the two halves of the jaw were then kept in apposition. By this method he hoped to obviate the bad effects of removing the jaw in its whole thickness. In this case, although the patient died of acute bronchitis, the result of this modification was satisfactory.

(4.) A CALCULUS, pencil-shaped, which he had removed from the female bladder. After dilating the urethra by ordinary bougies, he introduced a pair of forceps, and, grasping the calculus in its long axis, extracted it. Its shape at first suggested that it was a foreign body introduced; but a section showed that it was crystalline throughout its entire thickness.

(5.) A SIMPLE MEANS OF TYING IN A CATHETER, when the ordinary eyelets are awanting. This is effected by fitting on a small piece of indiarubber tubing to the proximal end of the catheter. A double thread is then passed through the indiarubber at one side so as to lie between the inside of the tube and outside of the catheter. The thread thus passed is next tied firmly round the tubing, and by this means it is firmly secured to the catheter. The two free threads forming each end are then knotted, and to the loops thus formed the ordinary tapes are fastened. A plug can then be inserted, and, owing to the elasticity of the tube, will always accurately fit and prevent any escape of urine. Mr Annandale further remarked, that as far as he was aware, this simple method had not yet been described, but that on this point he would be glad of any information. Dr Watson observed that he had already used this method.

II. *Dr Littlejohn* showed (1.) A HEART which he had removed from a man killed in a railway accident. He was a railway official, and, attempting to leap on a train in motion, he fell between the carriages. Both the wheels of one carriage passed over his body. What was remarkable was, that although the internal injuries were aggravated, the external skin was entire, and only here and there abraded. The heart, which was lying loose in the cavity of the chest, had been cleanly removed from its attachments as if a knife had been employed. It was firmly contracted and empty. The ribs on both sides were shattered. The spine was fractured and separated, and the left lung was ruptured. The diaphragm was intact, but the liver was ruptured at its base.

(2.) THE LUNGS, from a child aged three days. It was illegitimate, and had died suddenly. There was marked evidence of acute pleuro-pneumonia, and the greater portion of the lungs was condensed, yet the entire thoracic viscera floated freely in water. In this case, the "infarctus" of the tubules of the kidneys with uric acid was unusually well marked, and specimens were exhibited to the Society.

III. *Dr Watson* remarked, before proceeding to the exhibition of specimens, in response to Mr Annandale's request, that he had for long been in the habit of employing the method of tying in a gum-elastic catheter, recommended by Mr Annandale. He might remark, that in addition to the use of this plan, he could recommend that the indiarubber should be long enough to pass over the bedside so as to depend into a chamber-pot, and to form a few coils in the bed, so as to admit of the movements of the patient. Upon the tube he also employed a clip, such as is used for stopping the flow in volumetric analysis, so as to admit of the patient relieving himself without rising or altering his position.

Dr Watson showed (1.) THE FOOT OF A PATIENT, in whom, in March 1874, he had removed the scaphoid, cuboid, three cuneiform bones, and base of the metatarsal bones, in illustration of the amount and kind of consolidation which occurs after this operation.

(2.) TWO URETHROTOMES FOR INTERNAL URETHROTOMY, which Dr Watson had recently had constructed for him, in accordance with designs of his own. The instrument was essentially, in external appearance, the urethra staff for external urethrotomy, as employed by Mr Syme, with the addition of a concealed knife of the size of the common tenotomy blade, closing into the thin portion of the staff, and hinged at its base in the most distal part of the thick portion of the staff. The instrument was introduced closed. A screw in the handle caused angular protrusion of the knife, and the withdrawal of the instrument effected the division of the structure from behind forwards. The reverse movement of the screw caused the retreat of the knife into its groove, and then the staff could be passed onwards so as to determine if the thick portion passed readily through the constricted part. Dr Watson mentioned that the use of the instrument required no force and gave but little pain, and was attended with hardly any bleeding, while a No. 12 catheter could be passed immediately after its employment The first of these urethrotomes now exhibited, Dr Watson had made for him by Matthieu of Paris; but as its size (No. 4) at the probe-pointed extremity was much larger than suited his views, Dr W. had the slender, lighter, and more suitable instrument constructed by Mr Young of this city.

IV. *Dr Thomas Keith* showed an OVARIAN TUMOUR which he had removed from a patient in the fifth month of pregnancy. It was of rapid growth, and had been tapped three weeks before operation. The patient had done well. He further drew the attention of the members to the fact that he had now performed ovariotomy 190 times, with a mortality, during the last three years, of under ten per cent.

V. *Dr A. Sinclair* showed A PREPARATION, consisting of the pharynx and upper part of œsophagus of a patient who had died from hæmorrhage, resulting from injury done to the œsophagus and pharynx, by accidentally swallowing a gold plate of upper jaw, to which three teeth were attached. He saw the patient half an hour after the accident occurred, and found the plate with its convexity downwards firmly fixed in the upper part of œsophagus, a sharp hook-like process at one end being embedded in left side of pharynx, external to aryteno-epiglottidean fold. It was found impossible to extract by direct traction with œsophagus forceps; but after having dislodged and somewhat altered the position of the plate, he with great difficulty, having to use a considerable degree of force, succeeded in extracting it. A considerable amount of hæmorrhage occurred at the time, but this he succeeded in checking. He saw him early the next morning; a considerable quantity of blood had been expectorated during the night, but this had ceased at the time of his visit. He was sent for hurriedly during the forenoon, but by the time he reached his residence it was found the patient had expired. His attendant said that ten minutes before his death he had vomited about a quart of blood, and while in the act of taking some beef-tea, had sunk back in the bed and died. On examination of the pharynx after death, there was seen a large rent external to left aryteno-epiglottidean fold, and an abrasion on upper part of right side of gullet. Both aryteno-epiglottidean folds were much congested and swelled. A considerable amount of inflammation had taken place around the left lingual, external, and common carotid arteries. From the position of the wound, he believed the hæmorrhage came from a branch of lingual, although he was unable to trace its exact source.

VI. *Dr Littlejohn* next read a paper on two cases of ACCIDENTAL SUFFOCATION, which will appear in this Journal for February.

Dr D. R. Haldane commented on the clearness of Dr Littlejohn's account; and in regard to the evidence of Dr Joseph Bell, in the first of the two cases, remarked on the great superiority of a skilled medical observer over that of even experienced detectives.

VII. *Mr Annandale* then made a further communication in regard to A SUPPOSED CASE OF RENAL CALCULUS. The chief points to which he drew attention were the well-marked nature of the symptoms, the safety with which, by means of antiseptic precautions, he was enabled to cut down on the kidney; and also that although he had been unable to find any calculus, the distressing pains had been effectually removed by the operation. This last fact he thought could be explained by supposing that the incision acted as a strong counter-irritant.

Dr D. R. Haldane remarked on the boldness and interest of the operation. In regard to the cure, he mentioned that the late Dr Abercrombie was accustomed, in cases of intractable headache, to recommend that an incision should be made down to the bone and kept freely open for some time, and that the result of this was not unfrequently beneficial.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIII.—MEETING XV.

Wednesday, 8th July 1874.—Dr MATTHEWS DUNCAN *in the Chair.*

I. *Dr Macdonald* read a paper on the NATURE AND TREATMENT OF DIFFICULT OCCIPITO-POSTERIOR POSITIONS OF THE HEAD, which appeared at page 302 of the October number of this Journal).

Dr Wilson said there were so many points for discussion in Dr Macdonald's valuable paper that he felt a difficulty in knowing which to select. He agreed with Dr M. that in a large proportion of cases the head enters the brim more in the transverse than in the oblique diameters. He believed that cases of left occipito-posterior presentation which did not undergo the anterior rotation, and which terminated unaided with the face forwards, judging from his experience, were by no means rare. He had seen a good many such. In most of these cases, he believed that the child was either small or the pelvis large and the pains powerful. A large number of instrumental cases arose from the head not having taken the turn, which might arise from a variety of causes—*e.g.*, a large head, a deep narrow pelvis, or from ossification of the coccyx, combined with insufficient expulsive power. He believed that laceration of the perineum occurred very frequently, independent of the use of instruments. He had seen laceration of the vagina occur when no instruments had been employed, the mucous membrane alone giving way, while the skin remained entire. He had seen no evil result from these lacerations, although one or two had annoyed him at the time a good deal, especially those when forceps had been used.

Dr Young remarked that he had seen many cases in which the mucous membrane of the vagina had been lacerated in primiparæ, especially when the patient was above thirty-five years of age, and more especially when forceps were used, and the head large. Sometimes primiparous patients were very easy. He had not adopted Dr Macdonald's method of examining the lesions of the vagina, as he considered such interference to be wrong, unless where the case was severe, and the formation of fistulous openings into rectum or bladder might be feared. After the termination of the labour he did not consider it right to introduce the hand, as the patient, above all things, required rest, unless any portion of the placenta or membrane were left in the uterus.

Dr Duncan said that he experienced the same difficulty as Dr Wilson; so numerous were the points in the theory and treatment of head presentations as brought out by the paper read. Any one of them would be sufficient to occupy a whole night. As regards the practical remarks, he was at one with the writer, with one exception, namely, the introduction of the finger into the anus, for the purpose of catching the forehead above the supra-orbital process and pressing the head downward and forward over the perineum. Such a practice he considered neither safe nor desirable. Its principal use was to enable us to ascertain the state and position of the head. It was often much easier to get the finger up to the glabella through the anus than through the vagina. In this way it was possible to obtain an exact diagnosis of the position of a child's head, the position of the anterior fontanelle, and the coronal suture. The propelling force might, however, be attained by pressing upon the forehead externally. In many cases, they would find that the sacro-sciatic ligament was much distended. They could feel the child's forehead forcing back the ligament at the side of the coccyx. The forehead can be felt pressed down in this position. Sometimes one can effect delivery by pressure externally here with far greater power, because you can use the whole hand and not a single finger. By this means one avoids injuring the rectum, for recto-vaginal fistula has been produced by the finger in the rectum. He had seen a case in which the result had taken place probably by such interference at the end of labour. The finger internally might be employed to find out the exact moment for catching the head externally, as by catching it at the proper time the interference might become more efficacious. Dr Duncan was satisfied that laceration caused by the forceps was more dangerous than the common tearing of the perineum. Dangerous injuries sometimes took place in the region of the nymphæ, in the tissues surrounding the descending ramus of the pubes. These injuries he considered dangerous, because large vessels, especially veins, were generally opened into. In regard to what was said regarding the rarity of the fourth position or the left occipito-posterior, he considered it to be more common than Naegele would lead us to believe. As to the Solayres obliquity, the mechanical arrangement of pregnancy not only explained it, but rendered it necessary. In the majority of women the uterus lies in a peculiar position, with an inclination to the right. The fœtus lay with its back to the left side, and a little forward. The uterus was a little flattened, and had the left side tilted forward, and one might easily perceive how Solayres obliquity would occur.

Dr Macdonald briefly thanked the Society for the manner in which his paper had been received. As regards manipulation by catching the forehead through the rectum, he had not found any injurious results to flow from it; but he merely exerted passive pressure, as he held, that if due care were not exercised, not only might a recto-vaginal fistula be established, but in a first case perineal tear was very apt to be occasioned. He had, however, to thank Dr Duncan for the suggestion of external manipulation.

SESSION XXXIV.—MEETING I.

Wednesday, 18*th Nov.* 1874.—Dr J. MATTHEWS DUNCAN, *President, in the Chair.*

I. *Dr Jamieson* showed a FUNIS WITH A DOUBLE KNOT on it, but which had not interfered with the circulation through the cord.

II. The *President* exhibited a most interesting specimen of EXTRA-UTERINE FŒTATION. He reserved his description of it, and remarked, that though the woman denied being pregnant, the true condition of matters had been diagnosed.

III. *Dr Macdonald* read a paper on a DIPROSOPOUS TRIOPHTHALMOUS MONSTER, which will appear in a future number.

Dr Keiller was much interested in Dr Macdonald's paper. The monster shown was of very rare occurrence. It would have been of more scientific importance if Dr Macdonald had not stopped his admittedly careful dissection where he did, but given us also a minute dissection of the head. Doubtless, this arose from a not unusual cause, our unwillingness to over-mutilate a good specimen. Dr Keiller had a large number of monsters in his museum, more particularly the less uncommon forms of anencephalous monsters, with spina bifida. He alluded also to the prevalence of other deformities in different parts of the mesial line, such as cases of cleft palate, hare-lip, and exomphalos, of which he had many specimens. In reference to the position of the stomach in Dr Macdonald's case, Dr Keiller remarked that it was well known that this viscus at the earlier periods was more vertical than transverse, which might explain the great facility with which vomiting took place in young children.

The *President* was also much interested in the subject. He always laid stress on a minute dissection of monsters. He had never met with a case where the viscera were naturally arranged. He desired to point out that these cases had most important bearings on the doctrine of evolution. There were sports in animals as well as in plants. It would be interesting to know whether in the case of a monster with perfect genital organs the eccentricities or departures from the normal type might or might not be reproduced, or whether something new might not be the result. He rather thought that the anatomical arrangements in the thorax would debar life and reproduction in such a case as Dr Macdonald's.

Dr Macdonald thanked the Society for the manner in which his paper had been received. He admitted that, in his dissection, he had stopped short at an important point; but he had explained that his reason for not dissecting the face was, that he might possess in his museum this unique specimen with the face unmutilated. He would, however, consider the suggestion made to him by Dr Keiller. He held it was the duty of every one who chanced to fall upon one of these rare specimens to record and describe them as accurately as possible, in order to afford data for some one, who was certain some day to arise, able to explain the laws which regulated these departures from the normal type.

IV. *Dr Cairns* read a paper on the RISKS RUN BY OBSTETRICIANS, which will appear in a future number.

Dr Cochrane pointed out that the author, in his paper on an important subject, had omitted all reference to treatment. To the practitioner, treatment was of the highest importance.

Dr Cairns replied that the treatment pursued was, in the main, mercurial; and the preparations were the bichloride of mercury and Donovan's solution. He recommended the protection of the examining fingers by means of gutta-percha.

Dr Keiller said that in such a case as the one recorded by Dr Cairns (where a practitioner was infected by a serious disease), the medical man was much to be sympathized with; but as regarded the question of punishing the woman, there might be some doubt, and for this plain reason that she was often unaware of being the victim of such a disease. Where females knew of it, and did not make it known, there could only be one verdict, and that was to condemn, if not punish them. Dr Keiller had frequently been rather alarmed from digital abrasions which gave him annoyance. He begged to call the attention of the Society to the great practical importance of accoucheurs being ambidextrous, and to a rule he had adopted, viz., to use one hand chiefly for obstetrical, and the other hand for gynæcological purposes. By adhering to this rule it was obvious that mischief might frequently be prevented.

Dr Dickson suggested the employment of collodion, as an excellent protective in such cases.

Dr Macdonald observed that all obstetricians were more or less liable to such dangers as formed the subject of the paper, and more particularly those engaged in dispensary practice. He had been frequently, when summoned to operate in suspicious cases, seriously alarmed if he happened to have an abrasion. He had great confidence in the employment of unguents, not so much to facilitate examination as to prevent infection of the examining finger. But these must be supplemented by careful ablutions and the use of disinfectants such as carbolic acid or Condy.

Dr Burn stated that he knew of a medical friend who was infected with syphilis in the course of his obstetric practice. The case proved fatal. The practitioner's wife also caught the malady.

Dr Duncan said he was sure that the Fellows of the Society were much indebted to Dr Cairns. He knew two physicians who had suffered in the same way, and had their health completely ruined. One was an eminent surgeon. He had a sore on the index finger of the right hand, but he refused to recognise its character until compelled by the appearance of secondary symptoms. Dr Duncan was of opinion that it would be proper to make the concealment of this disease, under certain circumstances, a punishable offence. He had expected that Dr Cairns, in his paper, would have embraced some of the other risks to which obstetric practitioners were exposed. These were numerous. There was not only the risk of syphilis, but erysipelas, and various septic disorders. Siebold alleges that rheumatism is the peculiar disease from which the obstetric practitioner suffers. There could be no doubt but that this disease was brought on in many cases, and the same was true of affections of the chest, such as bronchitis. He was most decidedly of opinion that medical men in feeble health, and those of advanced years, should, if they regarded their own safety, decline altogether obstetric practice, and leave it to younger and more robust men. The female in labour might be an object of pity; but he was sure also that the aged practitioner, doomed to a protracted sitting beside her, was equally if not fully more entitled to commiseration.

V. *Dr Hardie* read an interesting paper, on PRICKLE CELLS IN THE EPITHELIUM OF AN UTERINE FIBROID, and exhibited microscopic illustrations.

Part Fourth.

PERISCOPE.

DR DOUGALL'S TREATMENT OF LEPROSY WITH GURJUN-OIL.—We have great pleasure in being able to continue the favourable report on this subject published in our July number. The treatment has now been carried on for seventeen months with unabated success, and various eyewitnesses have during that time borne independent testimony in its favour.

Dr Vincent Richard writes of it as follows:—"Dr Dougall had about thirty lepers under treatment at Haddo Hospital since 23d May 1873. When I saw them, they were all in, or rather had been in, different stages of the disease, but principally the last. Dr Dougall's treatment with the oil of the gurjun tree (*Dipterocarpus lævis*) is, so far as I could judge, a very decided success. I saw men, who a few months previously could only just crawl, run with ease, and pick up things with the greatest facility. Some of them informed me they had been under treatment for years, but without benefit, until Dr Dougall adopted his present mode of treatment. One, an old man, who had been very ill, described to me how he had been brought to hospital scarcely able to stand, with ulcers about his feet and toes, great thickening about his hands, tubercles over his face and body, general anæsthesia, and thickened voice, and how he had gradually got better under Dr Dougall's treatment. He started off running, to show me how active he was. There was little thickening about the hands, the ulcers were all healed, the tubercles had nearly all disappeared; he had recovered sensation, and his voice was perfectly clear. If the effects of Dr Dougall's treatment are permanent, he will have conferred on humanity one of the greatest benefits that has ever been conferred; and even if they are not, temporary relief from such a fearful malady is an inestimable boon to the wretched sufferers." The above was written in October 1873.

Dr Lethbridge, writing 17th May 1874, says:—"I had read Dr Dougall's paper on the subject in the *Medical Gazette*, and heard a very favourable account from Dr Lynch of the cases he had seen, but I was not prepared for the remarkable results which I actually witnessed. The belief in the efficacy of this treatment is universal, both among the Europeans and natives, in the settlement. I had accidentally the means of testing the opinion of prisoners on the subject on the morning of my arrival in the island. While being carried up to Mount Harriet by some prisoners, I noticed that many of the large trees in the jungle were deeply notched. On asking the prisoners what this was for, I was informed that it was to collect gurjun-oil for the treatment of lepers. I was surprised to find how unanimous they all were as to its being a great cure. One man, a Sikh, thinking I was incredulous, said, 'Why, sahib, if you don't believe us, you can see for yourself by going across to the hospital at Haddo.'

"The day after my arrival, I visited the Leper Hospital with Dr Dougall. I think I saw in all about twenty-eight cases. This is, I believe, the whole number of cases in the settlement. The majority were cases that Dr Dougall has already fully described in his paper; but two or three, suffering from the disease in its early stage, had been recently admitted. The patients were all paraded in a line, each man on his own mat, and having a quantity of gurjun ointment before him. They were going through the regular morning's operation of rubbing on the ointment. All the arrangements about the place were admirable; and what surprised me most, was the cheerful look the lepers had, and the vigorous way in which they rubbed the ointment. As far as I could judge, they had all, without exception, recovered their natural tone of voice, or rather, I should say, lost the voice so characteristic of leprosv.

"Dr Dougall tells me that this improvement of the voice generally precedes all other signs of recovery. The patients who had suffered from anæsthesia and partial or total loss of power over their hands and feet, had recovered sensibility and complete power over their limbs: men who could not hold walking-sticks being now able to handle a pickaxe vigorously; those who could not walk when first treated were made to run before me. Out of the whole number under treatment I saw only two with ulcers; these were situated on the sole of the foot, and were evidently healing rapidly.

"The most curious feature connected with this treatment is the process by which the tubercles disappear. They gradually as the treatment proceeds, become loose, and detached from the surrounding tissue, then get soft, and burst externally—the ulcers thus caused healing rapidly on the application of the ointment.

"The men complained that since they have been under treatment their appetites have improved so much that the diet allowed does not satisfy them. With regard to this, Dr Dougall explained that he did not wish to make a difference in this particular diet just at present, lest the benefit derived from the treatment should be ascribed to generous diet. In employing this treatment, therefore, with a liberal quantity of good nourishing food, we may reasonably hope to have even better results than those already obtained. As there has been no intermission in the treatment since it was begun, it is impossible to say whether the disease is entirely cured or only checked for a time. Dr Dougall proposes to carry on the treatment for a whole year, and then to test whether the effect is permanent. Whatever may be the issue of this further experiment, I think the results already obtained are so valuable that it is the duty of the profession now to urge on the Government the building of leper asylums in connexion with all stations. The treatment is so much appreciated by lepers themselves, that it would be no hardship if a compulsory order for segregation were issued.

"Leper asylums under the present treatment would be very different establishments to the loathsome places they were before.

"I think it is only fair to Dr Dougall that those who intend to try his treatment should carry it out to its full extent.

"The great point that he insists on is, that the ointment should be carefully rubbed into all parts of the body, taking care to remove all traces of old ointment, by rubbing the body with dry earth and then washing. I think it is important to mention this, for I was recently in a hospital where the treatment was supposed to be in use, and where I found the patients only rubbing the ointment on the parts affected by leprosy. I have for twenty days had eight cases under this treatment: the result so far has been most encouraging."

We subjoin a copy of Dr Dougall's latest Report to Government, through General Stewart, C.B., Chief Commissioner of the Andaman and Nicobar Islands, dated 8th July 1874, and resolution of the Government of India, issued at Calcutta, following thereon:—

"In the report on the treatment of leprosy with gurjun-oil, which was dated 23d November 1873, I entered fully into the question as regarded the then condition of the lepers, after being under this treatment for six months.

"2. As they have been now under treatment for thirteen months, I deem it my duty to bring to your notice the present condition of the men.

"3. As you are aware, the lepers in this settlement do not receive, and never have received, since they were declared unfit for ordinary labour, the same diet as the labouring convicts; they only receive the reduced diet, which is deemed sufficient for female convicts and men of the invalid gang. This reduced diet, though sufficient for the two classes just mentioned, is, in my opinion, not quite sufficient for the lepers under present circumstances, as most of them are not old or weakly men, neither are they accustomed to an idle life, as was formerly the case, when their disease, as a rule, precluded them from all active exercise on account of their bodily condition; the state of paralysis, in which most of them were, rendered it impossible for them to take any active exercise; and, apart from this, the numerous leprous sores from which they suffered rendered it inexpedient for them to be employed in any way with the other convicts, even had they not been paralyzed. All this is changed now, and almost every one of them is able for some amount of labour, which they have been performing under the direction of the apothecary Mr Phillips, such as trimming the ground around the hospital, improving the footpaths, etc.; and the following fourteen of the lepers being quite fit for labour, I have the honour to recommend that they may be employed at Haddo station on the lighter forms of employment, such as watchmen, etc.

" (*Here follow the names of the men.*)

"4. The above men are far more advanced towards what is ordinarily called a 'cure' than ever I expected to see them; and it is exceedingly gratifying to find that not only has the progress of such a disease been arrested, but I may without exaggeration say it has been obliterated. Traces of the disease are still apparent in the loss of fingers and toes, which of course cannot be restored, but otherwise these men are in the enjoyment of ordinary health and comfort. Where fingers and toes have been lost, the ulcers which formerly marked their sites are now perfectly healed cicatrices, and show no inclination to break out again into leprous sores.

"5. These fourteen men, if employed on light work as I have recommended, should, I humbly suggest, be restored to the grade they occupied before their transfer to the leper ward at Haddo. This would put them on a better scale of rations, and, in some instances, enable them to obtain such little luxuries as convicts may legitimately enjoy in this settlement; and by employing them at Haddo, they could be made to present themselves twice a week before the second medical officer, who would be in a position to observe any indications of a return of the disease.

"6. The remainder of the lepers not quite fit for employment, away from medical supervision and treatment, should, I think, get the same amount of food as an ordinary labouring convict, because they are all, without exception, progressing favourably and satisfactorily; and I am firmly convinced that even the very worst cases, such as Gunga Ram, Ram Ray, and Ravooth, only require time, as the improvement they have made from the commencement of the gurjun-oil treatment is most marked, and, though slow, it appears to be none the less sure; and in no single instance has there been an interruption in the steady progress they have made, further than that caused by the incidence of other diseases to which lepers are liable as well as others, such as attacks of fever, bronchitis, etc.

"7. Should you carry out the suggestion I have made, and employ these fourteen men in the manner recommended, it would be a great assistance in determining a not unimportant point that presents itself, viz., the *permanency* of their improved condition when withdrawn from medical treatment, as I would stop the use of the gurjun-oil in all the fourteen cases; and while not losing sight of them, I would be able to determine whether the improvement in their condition would be at all permanent, in spite of the discontinuance of the gurjun-oil.

"8. So far as thirteen months' experience enables me to form an opinion of the permanency of the improvement while the lepers are under the influence of the gurjun-oil, I can speak very confidently indeed, as in no instance has there been a relapse; but, on the contrary, I have to record nothing but steady progress, which is especially remarked in the case of lepers in whom the disease had not progressed very far before they were observed and brought under treatment.

"In such cases, the treatment, when regularly carried on, displays its efficacy in a very few months, as shown in the last five men on the above list, who all came under treatment since 19th October 1873.

"9. As yet I have not tried to deprive any of the lepers of their usual supply of gurjun-oil, and therefore cannot speak as to the permanency of their improved condition when not under its influence; but the recommendation contained in this letter regarding the employment of the fourteen recorded cases, if approved of by you, will afford a favourable opportunity for so doing, and help to solve a problem that is not without interest.

"But even if lepers had to persist in the use of gurjun-oil in order to ward off any further advances of the disease, this would be no great hardship to endure, with such an object in view; and as you have taken such an interest in their progress, and observed them frequently throughout the past thirteen months, I feel less diffident than I otherwise would, in bringing the whole matter in detail before you, for your favourable consideration."

Resolution of the Government of India:—Recent reports received from the Andamans afford additional evidence of the great value of the gurjun-oil as a cure for leprosy, and the Honourable the President in Council understands that, while it still remains to be seen how far the good effects may be permanent, there can be no question as to the great benefit to be derived from the use of the remedy in cases of leprosy.

The importance of this discovery to the natives of India, and not only to the natives of India but to all the many who suffer from the disease in various parts of the world, can hardly be overestimated. The President in Council desires therefore that the widest publicity may be given to Dr Dougall's mode of treatment, and that every endeavour may be made to extend its benefits throughout India wherever opportunity offers, and wherever the medical agencies at the disposal of the Government can be employed to assist the promotion of this object. The hospitals and dispensaries might collect patients from the districts, and the system should more especially be tested in any leper villages or asylums which may exist in the different provinces. The first step is of course to obtain a supply of the oil in a pure state. The principal medical store-keeper at Calcutta has made arrangements for obtaining large supplies from the Andaman Islands, and indents should be made upon him for its distribution. For the rest, the instructions given in Dr Dougall's Report, with regard to the method of treatment, are so complete and clear in detail, that they can be followed everywhere without difficulty. The Honourable the President in Council invites the co-operation of all local governments and administrations towards the extension of the use of this valuable medicine, and requests that careful reports on the results may be submitted at the end of a year for the information of the Governmnt of India.

FOURNIER ON SYPHILITIC GUMMATA (*continued from p.* 383).

3D PERIOD.—*Stage of Ulceration.* This is the most important of all the stages which constitute the gumma; and it is of these among the least well known. At first, after the tumour opens, the discharge of fluid is very trifling. Often only a few drops appear, and ooze out rather than flow; sometimes no more than a small spoonful of fluid is evacuated. After opening, the tumour remains much the same size as it was before, on account of the small quantity of matter that has come from it. The nature of the evacuated fluid is, more rarely than is generally supposed, of a viscid, gelatinous, syrupy, transparent or semi-transparent gum-like character. Much more frequently it is a sero-sanious, yellowish fluid, *pyöide* rather than purulent, containing in suspension organic detritus and grumous debris of cellular tissue, fatty granules, oil, and blood globules, and leucocytes. Yet again, but more rarely, it is a distinctly purulent fluid, yellowish-green, or still oftener a reddish, unhealthy pus. The gumma once opened, its aperture tends to enlarge. The small opening on its summit enlarges more and more by ulceration of the surrounding skin, until after some days it has the diameter of a lentil, a twenty-centime piece, or greater still. This circular orifice occupies the most central and prominent position. On examining the interior of the cavity, a solid, whitish, fleshy substance presents itself, resembling the flesh of a cod. This substance has the appearance of dead tissue, is without feeling, becomes disintegrated, and fibrilliform; portions the size of a filament of vermicelli become spontaneously separated from the mass. This substance is the actual tissue of the tumour, having passed, or passing, into the condition of a slough. The largest portion of the tumour is made up of this solid mass, and it is for this reason that the evacuation of fluid is so small, and the diminution in size of the tumour so slight.

The progressive elimination of the heart of the gumma proceeds thus:—From day to day, portions of greater or less size are detached from the mass by suppuration; and after the total evacuation of the core, all that is seen is the loss of substance sustained by the healthy tissue. This loss of substance, more or less deep, constitutes a real cavity dug out of the healthy tissues, and has received the name of a *caverne gommeuse.* Then by progressive ulceration of the edges of this cavity an ulcer soon forms of variable dimentions, according to the original size of the gumma.

The ulcer is characterized by being excavated; its borders are sometimes sharply defined and cut out, or prominent, and raised by infiltration of gummy products. It is surrounded by a dark-red areola, and furnishes a pretty abundant sanious or sero-sanious discharge, mixed with organic detritus.

So far, there is nothing special about its appearance, nothing that may not be observed in a variety of ulcers, and notably among tertiary syphilitic ones. The floor of the ulcer has an unhealthy aspect, is irregular, broken up, with depressions here and there, and covered with adherent detritus and sloughy putrescent particles like a wound partially affected with gangrene. On account of these divers characters and its unhealthy appearance, the gummatous ulcer may be mistaken, at first sight at least, for a cancerous ulcer. This appearance does not last long, for with slight attention the gummatous ulcer soon cleans itself and enters the stage of reparation.

4TH PERIOD.—*Reparation.* When the gummy debris is eliminated, the ulcer becomes clean, and takes on an improved aspect. The edges flatten, the red areola diminishes, the floor becomes raised and covered with fleshy granulations, the discharge gets thicker and more like pus from simple sores. The resulting cicatrix is at first red, gradually, however, becoming white, and always being depressed on account of the irreparable loss of substance. Such is the normal evolution of the gumma, which may be summed up in a few words.

1ST PERIOD.—The gumma appears under the form of a solid, hard, indolent, non-inflammatory tumour.

2D PERIOD.—The tumour becomes soft and fluctuating. A subacute inflammatory process attacks it; its surface reddens like a ripe abscess.

3D PERIOD.—The tumour opens, ulcerates, and discharges its core; then by destruction of its cutaneous investment it transforms itself into an open ulcer of unhealthy aspect.

4TH PERIOD.—The ulcer becomes cleaner, repairs itself, and cicatrizes.

What is the duration of a gumma?

1st, The duration is always long; three to four months at least; usually six to eight months, sometimes longer.

2d, The duration is very variable. The variations occur during the two first periods, more especially during the first of these; for the two last (in general, at least, with exception of certain cases, to which reference will presently be made) evolve themselves in a tolerably equal, regular, and relatively rapid manner.

The first period, or period of formation, is the most unequal in its duration. There are cases in which the matured gumma remains for a long time stationary without perceptible modifications. The second period is also irregular. There are some gummata which soften rapidly, fluctuating as soon as formed; while in others softening occurs very slowly and partially in isolated parts of the tumour.

When once peripheral inflammation shows itself, things proceed tolerably quickly and regularly in all cases. A few weeks suffice to empty the gumma and change it into the condition of an ulcer. With a little simple local treatment, the cleaning and reparation of the ulcer goes on in a somewhat rapid manner.

VARIETIES.—What has just been described is the gummy tumour under its simplest form, unaffected by any accident or complication. It is subject, however, to some varieties of symptoms and evolution, which require to be pointed out. Amongst the variety of symptoms, two points alone require to be noticed.

1st, The gummy tumour has been represented as essentially indolent, causing no pain, and being insensible to pressure. This happens in the majority of cases, at least ninety-five times out of one hundred. But it should also be known that in certain cases the tumour, so far from being indolent, gives rise to sharp pain, which is sometimes even violent and excruciating. This pain is produced when the tumour either compresses or invades a nerve trunk or branch. This occurred in a case of M. Ricord, a gummy tumour of the groin giving rise to painful crural neuralgia, which extended over the whole thigh from the groin to the knee; in another case, by the same author, two gummata, situated over the course of the ulnar nerve, provoked sharp pains radiating from the forearm to the last two fingers, thus following the distribution of the nerve.

2d, In most cases the gummy tumour is not in itself the cause of any functional disturbance. But it is otherwise, as when, for example, a large tumour, by its volume alone, becomes the origin of functional derangements; or when a tumour, situated over a movable part like the jaw or hand, interferes with the movements. But this is of secondary interest to what must now be considered, viz.:—

VARIETIES IN NUMBER.—There is usually observed only one gummy tumour, or at most two, three, or four gummy tumours in the same patient. It is very rare for this number to be exceeded; but in some cases, and especially in patients who have suffered from a severe form of syphilis, *multiple* gummy tumours are met with, and when they begin to multiply, they go on increasing with amazing facility. There may then be seen from eight to fifteen of them, unequal in size, disseminated or collected in groups, and of different degrees of development. One patient had a group of six tumours on his back. Another, a woman, presented four tumours on the head, three on the legs, two on the forearm, and one on the shoulder-blade.

We now leave the ordinary cases, and enter upon the exceptional. On some patients, gummy tumours to the number of twenty, thirty, thirty-five, and fifty, have been met with.

M. Corvisart has recorded a case where, besides several other incontestably syphilitic lesions, more than one hundred tumours occurred on the breast, belly, and arms.

Lastly, We shall describe some extraordinary cases.

In the ***Bulletin de Thérapeutique***, 1845, a case was recorded by Lisfranc, in which a patient presented one hundred and sixty gummy tumours on the arms, forearms, and thighs. "The smaller ones were the size of a small nut, the more developed as large as a small pear." Iodide of potassium was administered, and after eight months' treatment only forty tumours remained; the one hundred and twenty had been cured by resolution.

Still more important are the variations in size, which remain to be described.

VARIETIES IN VOLUME.—It has been shown that the gummy tumour has usually a small size, commonly that of a nut. The size may, however, increase to that of a hen or turkey's egg, and in certain rare cases to even a considerably larger size. To cite only two extreme cases, I have observed a gumma of the thigh as large as half the head of a fœtus. Dr Mendeville observed at Val-de-Grâce a gummy tumour larger still. It occurred on the head, and implicated the entire lower half of the face, a large part of the neck, and the whole inferior half of the skull. On account of the rarity of such cases, and their excessive size, these tumours are liable to be mistaken for growths of another nature, especially cancerous ones, and are thus exposed to the risk of grave surgical interference.—(*To be continued.*)

ON THE INFLUENCE OF MERCURIAL PREPARATIONS ON THE RICHNESS OF THE BLOOD IN RED AND WHITE GLOBULES. By Dr WILBOUCHEWITCH of Moscow. *Archives de Physiologie*, July—September 1874.—The researches recorded by Dr Wilbouchewitch in this paper have been made by means of the simple and ingenious instrument invented by M. Malassez, for counting with precision the number of red and white globules in a given quantity of blood. For a detailed account of the instrument, and how to use it, the reader is referred to M. Malassez's monograph, *De la Numération des Globules rouges de Sang*, Paris, 1873, chez Delahaye.

The principle of it consists in diluting with artificial serum a fixed quantity of blood to a hundredth or two hundredth. A drop of the dilution is transferred to a capillary glass tube of known capacity, and the number of globules covered by a square inserted in a micrometer eye-piece, with the tube of the microscope fixed at a given point and with a given objective, is counted. The product of the degree of dilution, the number of globules, and the figures corresponding to the capacity of the tube, which are engraved on the piece of glass on which the tube is fastened, gives the number of globules in a millimeter cube.

The result of observations made on seven adult males, the blood being taken before, half an hour after, and four hours after a meal, shows that the normal number of red globules contained in a millimeter cube of the blood of a healthy man during winter, varied between 4,200,000 and 6,477,000; that of the white globules between 6900 and 8550; the proportion of the white to the red globules varied between 603 and 757. Thus, the proportion is not one in 300, as has been supposed by many physiologists who had to be satisfied with imperfect methods.

(The table given by the author shows that of the seven healthy men, in six the extremes lay between 4,200,000 and 4,728,600.)

The influence of the administration of mercury on the number of red globules in the blood was studied on patients in the Midi Hospital, under the care of Drs Simonet and Heurteloup. Dr Simonet uses the bichloride, and Dr Heurteloup the protoiodide.

The patients had all infecting chancres, glandular enlargements in the groins, and some had secondary symptoms.

The observations were always made under the same conditions. The blood was examined four successive days before treatment was begun, and again several days after the mercury had been taken. He made 168 observations on ten patients, a number sufficient to exclude possible sources of error. These observations the author gives in detail.

The number of red globules was always found to be greater during the treatment than it was before, and more considerable during the first period of treatment than during the second. The mean of the different tables is given in the following figures:—

Periods.	Red Globules.	White Globules.	Ratio.
Before the treatment (40 numerations)..........	4,321,500	7,570	586
During the whole period of treatment (119 do.)	4,669,200	7,990	584
During the first period of treatment (60 do.) ..	5,015,400	6,820	735
During the second period of treatment (59 do.)	4,390,000	8,200	535
After treatment (7 do.)...............................	4,785,500	7,018	682

The conclusions drawn from a table, showing the influence of the mercurial treatment on the intensity and rapidity in the variations of number of the red globules in syphilitic persons at the beginning of the disease, show:—*Firstly*, That before the treatment the number of the red globules tended to a decrease; the mean of this decrease was 229,405 per millimeter cube of blood daily; the minimum decrease was 87,575, and the maximum decrease 510,150. *Secondly*, That the period of treatment during which the number of red globules augmented varied from nine to twenty-one days; the mean of all the observations gives sixteen days.

During this period, the augmentation of the globules was 82,525 per millimeter cube in the first eight days, and 121,890 in the eight days following; the general average of gain during the whole of this period of treatment being 102,450 red globules daily. *Thirdly*, A period of loss always supervening when the mercurial treatment was pushed beyond a certain point, it was found that the average duration of this period necessary to reduce the number to what it was when the treatment was begun, was twelve days. *Fourthly*, On the treatment being discontinued the diminution ceased, and after nine days, on an average there was an augmentation which could be estimated at 87,746 millimeter cube daily.

The following are the general conclusions arrived at by the author:—

1. Before treatment, the number of globules progressively diminishes, and descends below the average; syphilis is apparently a cause of hypoglobulie.

2. During treatment, the number of globules increases and becomes normal; it is difficult not to see in the constancy of this fact, the influence of the mercurial treatment, and not to conclude from it that mercury arrests and cures syphilitic hypoglobulie.

3. The treatment being continued, the globules diminish in number. Mercury given in too large doses, or during too long a time, causes anæmia; destroys the good it had done.

4. *Finally*, If, with cessation of treatment, the number of globules again increases, it is doubtless because, the mercury being gradually eliminated, the quantity in the system is reduced to proportions in which, far from being hurtful, it is useful. Dr Wilbouchewitch insists, with great justice, upon the value of M. Malassez's instrument in enabling the physician to determine at what point the mercurial treatment should be discontinued.

SUPPRESSION OF URINE.—Dr Tournadre communicates an interesting case of suppression of urine, existing for eight days, and during which time no symptoms of uræmic intoxication presented themselves. The patient, a man about fifty years of age, of good constitution, except being somewhat rheumatic, was suddenly attacked by suppression. When seen, he was not feverish, and suffered no pain, pulse and temperature normal. Dr Tournadre, taking into consideration the fact of his rheumatic tendency, and bearing in mind the possibility of gravel being present, ordered rest, prolonged use of hip-bath, and linseed-tea internally. On the fourth day, the patient was very ill; the abdomen tympanitic, some dyspnœa, accompanied by great depression; no headache, delirium, hallucination, or alteration in vision, etc. On examination of the bladder with a catheter, no urine was obtained, but some small gravel felt. Bicarbonate of soda was added to the tisane, and friction applied over hypogastric and lumbar regions. A little diarrhœa, which he has had from the beginning, continues. He continued in much the same state up to the eighth day, when he was placed in a hot bath for two hours, and while in it, a clot of blood escaped from the urethra, followed by a flow of normal urine accompanied by twelve or fifteen small calculi. The patient quite recovered.—*Gazette des Hôpitaux*, 17th October 1874.

VEYSSIERE has recently made some experiments in the localization of sensibility in the dog. The method he employed was to introduce a small trochar through a puncture in the cranium. The stylet was then withdrawn, and another, bearing a concealed spring, which could be thrust out at an angle from its extremity, was pushed down the canula, the spring projected and the requisite laceration made, the spring recovered, and the instrument withdrawn.

Experiment No. 1.—The lesion was limited to the posterior part of the intraventricular portion of the corpus striatum and the anterior part of the optic thalamus on the left side.

Result.—Imperfect hemiplegia and hemianæsthesia of the right side. The hemiplegia passed off in twenty-four hours; the animal was well in three days.

Experiment No. 2.—Lesion, complete destruction of the inferior three-fourths of the peduncular expansion, and of the posterior part of the extra-ventricular portion of the corpus striatum.

Result.—Nearly complete hemiplegia and hemianæsthesia.

Experiment No. 3.—Lesion, destruction of peduncle where it commences to expand (pied de couronne rayonnante de Reil) and a solution of continuity in the peduncle lower down.

Result.—Hemianæsthesia lasting ten days.

Experiment No. 4.—Lesion, section of the peduncular expansion (couronne rayonnante).

Result.—Encephalitis, hemiplegia, and hemianæsthesia; death.

Experiment No. 5.—Lesion, the middle part of the peduncular expansion and the extra-ventricular part of the corpus striatum were destroyed.

Result.—Incomplete hemiplegia and hemianæsthesia.

Negative results followed lesions of nearly the whole cortical substance, the corpus striatum, or optic thalamus. He concludes, therefore, that in order to provoke hemianæsthesia in dogs, you must wound the peduncular expansion.

CHANGE OF WET-NURSES.—In a lecture at the Hospital des Enfants Maladies, M. Bouchut *weighs* the "delicate" question of change of wet-nurses. By the scales you can determine to a gramme how the child is thriving, and whether the milk of the nurse is sufficient in quantity. Apart from the application of the facts M. Bouchut makes, the results he adduces are interesting and valuable.

In several, he found the average weights of children, and of the milk-supply of nurses, lower in hospitals than those of the town —a fact we recommend to the notice of parochial authorities. The weight of infants at birth varies from 3 to 4 kilogrammes; 5, 6, and 7 kilogrammes are exceptional.

Boys are heaver than girls. Male average 3 k. 20, female average 2 k. 91 (Quetelet and Winckel).

Multiparæ bear ordinarily heavier children than primiparæ; but as Duncan and Foisy have shown, the age of the mother has an influence on the weight of the child, thirty to thirty-five years affording the highest averages. Individual peculiarities in the parents vary the results. Many circumstances tend to lessen the weight of the child at birth: debilitated and lymphatic constitutions; frequent and abundant vomitings of pregnancy; scrofula, and, above all, syphilis; hæmorrhage, when considerable; and, according to Foisy, the presence of varices.

Chaussier first established the fact, confirmed by Siebold and Winckel, that an infant loses weight during the first few days of life. Bouchaud found exceptions to this rule in about one case in fifteen. From 30 to 100 grammes are lost each day for three days, after which, the weight again increases. The loss is less in private life, under good influences, than in hospitals or maternities and among the children of the abandoned.

At the end of the second day, a child will weigh 100 grammes less than at birth—a diminution which corresponds to the excretion of the meconium. On the seventh day the weight should be the same as at birth. From the latter period to the age of five months it should increase on an average of 175 grammes per week, or about 25 grammes per day. After the fifth month, it will not increase more than an average of 15 grammes per day. At the age of five months, the infant should weigh twice what it did at birth. At sixteen months old its weight will be only double of what it was at five months. These laws must not be too rigorously applied; some children may gain only 15 or 20 grammes per day, and be in excellent health. The important point is, that there be a steady gain of some grammes. To determine the quantity of milk, the child is weighed before and after each sucking. There should be from 80 to 100 grammes for each meal; when under 50, the nurse is insufficient. At one month the child should receive 630 grammes per day; in the second, 700 grammes; third, 840 grammes; and in the fourth, 950 grammes. These results are derived from hospital observation; in private they were greater. If the infant does not thrive, if its weight is not steadily increasing, or if the quantity of milk taken at each nursing is insufficient, the nurse should be changed. Temporary illness on the part of the nurse matters not, but if the disease is bad, or prolonged and likely to affect the milk, a change of wet-nurse is necessary.—*Gazette des Hôpitaux*, 7th July 1874.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Librarian to the Royal College of Surgeons in Ireland; Honorary Member of the Ontario College of Pharmacy, etc.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

ON THE ACTION OF EMETICS.—Two opinions are held as to the mode of action of emetics. Some authors believe that emetics by contact with the peripheric extremities of the pneumogastric nerves cause excitation of them, leading to the reflex act which produces vomiting. Another view is, that the only requisite for the production of emesis is contact of the agent with the medulla oblongata, where it can excite the origin of the vagus. M. Chouppe believes from his experiments, that different emetics act differently; some, as tartar emetic and apomorphia, act both on the central nervous system and gastric mucous membrane; others, as emetine, act exclusively on the peripheric extremities of the vagus.—*Gaz. Méd. de Paris*, 1st August 1874.

ACTION OF DRUGS ON SECRETION OF BILE.—Röhrig finds that of the purgatives the following is their order, according to their power of increasing the secretion:—Croton-oil, colocynth, jalap, aloes, rhubarb, senna, bitter salts, calomel, and castor-oil. These agents probably increase the secretion by a direct action on the liver after absorption, although they produce in the intestines a distinct hyperæmia. Injection of infusions of senna and rhubarb into the blood immediately increase the flow of bile. The power which these agents have of producing congestion of the intestine, and thus increasing the supply of portal blood, probably plays an important part in producing the increased secretion. Acetate of lead and carbonate of soda decrease the quantity of bile probably by decreasing the supply of blood to the liver. Opium increases the secretion of bile apparently by paralyzing the bloodvessels, and thus increasing the blood-supply. Röhrig confirms the observation of Schmulewitsch, that the liver removed from a living animal continues to secrete bile if warm defibrinated blood is passed through its vessels. —*Stricker's Jahrb.* and *Glasgow Med. Journal.*

NITRITE OF AMYL.—Dr J. Crichton Browne observes that nitrite of amyl, when inhaled during a state of unconsciousness, has a specific action on the motor centre of the mouth, and calls into action, by preference, the muscles of the lips and lower jaw. The mode in which it performs this action, whether reflexly or through the agency of the vaso-motor apparatus, is as yet only a subject of speculation. The fact that the movements which it evokes are consentaneous with the appearance of flushing of the face, gives probability to the latter hypothesis; while, on the other hand, one observation, that when administered hypodermically, the nitrite failed to induce yawning in a case in which it had induced it when inhaled, seems favourable to the other view. Whatever may be the explanation of the action in question, there can be no doubt that it is a significant fact, and worthy of further investigation.—*Practitioner*, Sept. 1874.

ACTION OF AMYL NITRITE.—R. Pick corroborates experimentally the statement that this agent produces dilatation of the vessels. Its action is exerted on the muscular tissue, and not on the nervous centres. It acts relaxingly on the vascular musculature. Pick observed that if on breathing the vapour the eyes be fixed on a spot on a bright wall it will appear surrounded by a yellow circle, outside which is a bluish violet area bounded by sinuous lines.—*American Journal*, 1874.

ATROPINE AS AN ANTIDOTE TO MORPHIA.—Dr S. Wordsworth Poole records a case of poisoning by morphia which was successfully treated by injection of one-sixth of a grain of sulphate of atropia. —*Practitioner*, Oct. 1874.

PHYSIOLOGICAL ACTION OF THE PREPARATIONS OF BROMINE.—In his researches on this subject, Dr Steinauer used hydrobromic acid, bromated acetic acid, especially the monobromo-acetic acid. Bromo-benzol and bromo-benzoic acid, hydrobromic acid, and bromated acetic acid and its salts, are poisonous. Doses of from 7 to 15 grs. proved fatal to rabbits in half an hour. Under the influence of hydrobromic acid the frequency of the pulse fell, the frequency of the respiration was increased, and the animals became quiet. Voluntary power was soon lost, and reflex activity was at first diminished, and ultimately lost. The respiration and cardiac beats then became irregular, convulsions then ensued, and then death. Post-mortem examination showed that the action of the heart was arrested in diastole. The symptoms induced by the monobromized acetic acid were similar, but the heart was found to be arrested in systole. From the fact that section of the vagus or paralysis of the terminal branch of the cardiac nerves by nicotine did not prevent the fall in the frequency of the pulse, Steinauer concludes that the vagi are not acted on by this poison. He thinks it causes paralysis of the cardiac muscle, with coincident affection of the excito-motor cardiac centre.—*Centralblatt für der Medicinisch Wissenschaften*, No. 26, 1874.

HYDROCHLORAL BY THE RECTUM IN THE VOMITING OF PREGNANCY.—In a case of excessive vomiting, when all other remedies failed, Dr Simmons recommended the administration by the rectum of thirty grains of hydrochloral in mucilage night and morning. The treatment was entirely successful. In future cases, Dr Simmons will give larger doses, as he is convinced that a decided impression at first will require the repetition of the medicine but two or three times. He believes that hydrochoral administered by the rectum will relieve most cases of sympathetic vomiting, especially when there is no inflammation; and in strangulated hernia it ought to act beneficially, both by checking the vomiting and by producing relaxation.—*New York Med. Record*, June 1874.

DIGITALIS IN PUERPERAL CASES.—Dr Winkel does not regard digitalis as a specific in puerperal fever, but he administers it as a prophylactic against the phlegmonous process. He discontinues it where the digestion becomes disturbed. He found the hypodermic use of digitalin, dissolved in equal parts of alcohol and water in the proportion of ·001 gramme of digitalin in 3 grammes of fluid, to be the best mode of administration. The mean requisite dose is ·005 gramme. The effect follows in one to three days generally, a simultaneous sinking of temperature and pulse being observable. Elimination cannot be absolutely stated to take place through any organ. Digitalin appears to act on the brain, producing rest, better sleep, and enlargement of the pupil. It appears also to act as a styptic, recommending itself in this respect after severe hæmorrhage. The question as to the tolerance by puerperal women of large doses cannot as yet be answered, the assertion of its only partial absorption through the altered condition of the intestine being unsupported by proof. The favourable action of digitalis is attributed by Dr Winkel to its effect on the circulation; for, as in the chills of the puerperal condition the prognosis is more favourable when the pulse is not over 100, it is evident that a remedy which will prevent such increase must be beneficial. The observation of after-pains following the continued use of digitalis, together with its styptic action, has given rise to the conjecture that its effect is upon the unstriped muscular fibres of the uterus. By its contraction of the capillaries it prevents exudation of the colourless corpuscles.—*New Remedies*, July 1874.

FERRUGINOUS BENZOATE OF LITHIA IN THE TREATMENT OF GOUT AND URIC ACID DEPOSITS.—Various works recently published have spoken in high terms of the use of benzoate of lithia in cases of gout and uric acid urine, and MM. Dalkiewicz and Mallez have recommended its use combined with ferruginous preparations. This is an instance of rational therapeutics, for benzoic acid in passing through the body undergoes conversion into hippuric acid at the expense of the proteic substances which are the principal source of uric acid. Hippuric acid forms with the ordinary bases of the body—as soda, potash, and ammonia—salts possessing a high degree of solubility, whilst the urates of these same salts are insoluble in the liquids of the economy, and form almost exclusively the gravel of the urine and the *tophi* of the joints. M. Gubler has hence given, in his "Commentaires du Codex Medicamentarius," the preference to the use of the benzoic acid. But this acid cannot be employed *per se* for a long time, as is shown by the experiments made in the Paris hospitals. M. Tréhyoa has therefore thought it advisable to associate lithia with benzoic acid, producing a very soluble salt, which prevents the formation of uric acid, and forms with that which exists a urate of lithia, which is the most soluble of all the urates. Like all the alkaline preparations, however, this, if continued too long, has its inconveniences, and M. Tréhyoa has had the excellent idea of adding iron to it. Experience has shown that he has not been mistaken, various writers having published statements showing the advantages which they have seen follow the use of the compound preparation.—*Le Progrès Medical*, 25th July 1874, and *Practitioner*, October 1874.

THE PHYSIOLOGICAL ACTION OF HYDROCYANIC ACID AND THE ANTIDOTAL ACTION OF ATROPINE.—The following are the conclusions arrived at by Dr Boehm:—1. The operation of prussic acid is directed upon the central nervous system, whose functions are annihilated by large doses, after a brief excitement or increase. 2. The lesions of respiration and circulation arise from analogous changes in the activity of their centres in the medulla oblongata. 3. The vagus plays no part either in the effect of prussic acid on the respiration, or in its effects upon the heart. 4. Atropine is not an antidote to prussic acid. The only rational treatment of this poisoning is the persevering performance of artificial respiration.—*Practitioner*, Sept. 1874.

HYPOCHLORITES IN THE TREATMENT OF VARICOSE ULCERS.—M. Panas regards as the essential feature of varicose ulcers of the leg a local asphyxia caused by congestion with stagnant venous blood. By local oxidation of the stagnant blood the ulcer is converted into an ordinary granulating surface. M. Panas observed that when hypochlorites, especially hypochlorite of lime, are applied to these ulcers a decomposition takes place, resulting in the evolution of free oxygen in the nascent state.—*Centralblatt für Chirurgie*, No. 14, 1874.

THE LOCAL ACTION OF IPECACUANHA.—Dr Noël Gueneau de Mussy communicates a brief paper on this subject to the *Practitioner*, Sept. 1874. He alludes to the fact that in chronic dysentery, and even in common chronic diarrhœa, injections of decoctions of ipecacuanha into the intestines are a common practice in Peru, and in some other countries of South America. The following is his own formula:—Ipecac. root, ʒi.; boil for ten minutes in water, ℥v. Let it infuse for one or two hours, strain off, and make use of the decoction as an enema. The successful result of the local application of the decoction in enteritis induced Dr de Mussy to try it in some other inflammatory affections of mucous membranes. In a case of purulent ophthalmia in a new-born child, after the acute symptoms had been treated by solutions of nitrate of silver, the ulcerations and opacity of the cornea and the granular conditions of the conjunctiva which remained, were successfully treated by the topical application of decoction of ipecacuanha, of the strength of ʒss. of the root to ℥v. of water. Dr Galezowsky has also tried the remedy with success in several cases of subacute inflammation.

Subcutaneous Injection of Calomel in Pneumonia.—The *London Medical Record*, 26th August, contains an abstract of a paper on this subject by Dr Achille Formenti, in the *Gazzetta Medica Italiana-Lombardia*, 8th August. Dr Formenti injected into the arm from one to three grains of calomel in fifteen drops of water, according to the age and special condition of the patient. He applied a few leeches in some cases over the seat of pain, but he did not bleed the patients, neither did he give medicines internally, except refreshing diuretic drinks. The treatment was followed by marked improvement. In no case did the production of abscesses or of salivation result, and no indication of hepatization was to be detected on examination some time afterwards.

Mercury as a Cholagogue.—In Dr Murchison's lecture on the functional derangements of the liver (*Lancet*, 2d May), he expresses his belief that mercury and allied purgatives produce bilious stools by irritating the upper part of the bowel, and sweeping on the bile before it has time to be absorbed. He adds that there are grounds for belief that mercury acts beneficially in many functional derangements of the liver. The good effects of mercury on the liver, and in some forms of inflammation, may be due to its property of promoting disintegration. Mercury apparently has the power of rendering effused lymph less cohesive, and so facilitates its removal by absorption. It is not improbable that mercury may under certain circumstances promote or influence the disintegration of albumen. Dr Murchison considers that the clinical proofs of the efficacy of mercury in certain derangements of the liver are overwhelming.

Nitrate of Lead in Onychia Maligna.—In the *Lancet*, 23d May, Mr Fairlie Clarke reports three cases of onychia maligna treated by nitrate of lead. He regards this remedy as of great value. In the *British Medical Journal*, 17th January, Mr Howard also records a case successfully treated with powdered nitrate of lead.

On the Use of the Alcoholic Extract of Nux Vomica in Large Doses in Various Forms of Nervous Diseases.—Dr de Stefani does not regard nux vomica as an irritant to the spinal cord, but believes that it exerts a depressing action on the ganglionic system. As this system has numerous relations and sympathies with the cerebro-spinal, nux vomica, acting on both, relaxes the vital tension of the nerves, restores to them their natural conducting power, and also the degree of influence necessary to maintain the harmony of the vital functions of the organs. In acute and serious diseases of the two nervous systems the tolerance of the drug is great; in chronic affections of the ganglionic system it is greater than in that of the cerebro-spinal; and in the organic diseases it is in relation to the gravity of the nervous sympathies. Intolerance of the drug is indicated by stiffness of the lower jaw and of the tongue, and some degree of subsultus in the lower limbs or in all the body. With reference to the curative action of nux vomica, Dr de Stefani maintains that it depresses the muscular force if this has been stimulated by hypersthenia, and stimulates it when it has been apparently depressed by the same cause; that it lowers the pulse when it is hard and vibrating, and raises it when it is small and weak; that it lowers excessive heat of the skin, and warms the skin when it is morbidly cold; that it regulates both the pulse and heat of the skin when they are variable several times in the day; that it relieves ardent thirst; that in costiveness which has resisted repeated purgatives it opens the bowels, and in some cases arrests diarrhœa; that it also arrests spontaneous hæmorrhage and relieves hæmorrhoids; that it relaxes spasms, removes neuralgic, pleuritic, and rheumatic pains, calms delirium, and removes morbid wakefulness, or awakes patients from morbid sleep, promotes perspiration when deficient or arrests it when profuse, etc., whenever these symptoms are the results of a nervous affection. Nux vomica should not be employed in nervous affections until other remedies have failed. When its use is decided upon it is necessary to guard against giving too small doses. The dose of the alcoholic extract given by Dr de Stefani to subjects of middle age suffering from chronic disease is from five to ten centigrammes. In serious cases this may be raised even to thirty centigrammes, combining with it an equal quantity of extract of rhus radicans and some extract of henbane.—*Le Sperimentale*, Firenze, May and June, and Dr Semple's report in *Med.-Chir. Review*, October.

PODOPHYLLIN IN THE TREATMENT OF ACUTE RHEUMATISM.—Dr R. F. Dyer, of Ottawa, Illinois, attributes a specific influence to podophyllin as a remedy in acute rheumatism. He commences the treatment by giving small doses, combined with Dover's powder, every two or three hours, until the bowels are freely moved. Relief sometimes follows two or three evacuations, but occasionally eight or ten are requisite. If after free evacuation relief is not obtained, he continues to give the medicine in divided doses. He usually follows the administration of podophyllin by the alkaline treatment.—*American Quarterly Journal of Medical Sciences*, July.

THE ABORTIVE ACTION OF QUININE.—The following are the conclusions arrived at by Dr William Goodell (*Trans. Med. Soc. of State of Pennsylvania*):—Quinia, by producing intermittent contractions of the womb, has, in large doses, occasionally brought on an abortion in the very early months of gestation. That it should not on that account, however, be withheld from pregnant ague patients; for, other things being equal, an abortion is more likely to be induced by the visceral congestions and muscular succussions attending an attack of ague than by the oxytocic property of the antiperiodic. That the uterine action of this drug is too slow and too uncertain to be relied upon in the emergencies of ante or post partum hæmorrhages. But that, in decided doses, it will often prove of service in menorrhagic or metrorrhagic attacks. That, like ergot, it acts most efficiently after labour has begun; a dose of ten grains being usually followed, in inertia, by a prompt return of the pains. That, apart from its tonic and antiseptic properties, quinine is *par excellence* the remedy for puerperal disorders. By lowering the high temperature generated both by accelerated molecular metamorphosis and by rapid chemical combinations, it retards the oxidation of the tissues, hinders the formation of fibrinous concretions, and therefore prevents cardiac plugging. By contracting the walls of the womb, it tends to keep the protective coagula of the uterine sinuses from becoming loose and soluble, and to inhibit putrid and purulent absorption. Both by constringing the coats of the capillary system of bloodvessels, and by paralyzing the amœboid movement of white blood-corpuscles, it presents, in puerperal fevers, an obstacle to fibrinous exudations and to the migration of the leucocytes into serous cavities.

USE OF QUININE IN THE TREATMENT OF INFANTILE DISEASES. —In a paper in the *Deutsche Klinik*, 1874, p. 167, Dr Rapmund contends that quinine and cold affusions are the most energetic and most certain antipyretics. He gave quinine in four cases of scarlet fever, eleven cases of measles, two cases of smallpox, three cases of erysipelas, nine cases of lobular pneumonia, and three of follicular enteritis. When a sufficient dose had been administered, the temperature and frequency of the pulse fell, and a calm prolonged sleep ensued. Quinine has great power in rendering the course of febrile diseases benign. In the treatment of erratic erysipelas by quinine, Rapmund has been as successful as Vogel. In the treatment of lobular pneumonia of infants by quinine, the author obtained seven cures out of nine cases. It is in the stage when the febrile symptoms are acute, and the temperature and pulse-rate higher than normal, that quinine is indicated. In hooping-cough, quinine diminishes the violence of the attack, and promotes rest at night; it also appears to prevent complication, and to render the course of the disease benign.

EXCRETION OF CARBONIC ACID AFTER INJECTION OF QUININE. —The following are the results of experiments made by Dr Strassburg:—1. Quinine administered to healthy rabbits in moderate doses lowered the temperature, but did not diminish the absolute amount of exhalation of carbonic acid. 2. Quinine administered to rabbits in a febrile state caused a diminution in the amount of carbonic acid eliminated.—*Archiv f. Experiment. Pharmakologie*, ii. 334.

THERAPEUTICAL ACTION OF QUININE.—The *Lancet*, 8th August, contains a summary of M. Sée's lectures, delivered at the Charité, on the therapeutic action of quinine. In health quinine diminishes the frequency and force of the action of the heart, it lowers the tension of the arterial system, and it lowers the temperature or prevents its elevation by exercise, etc. Its action on the amœboid movements of the white corpuscles he does not consider as of importance. M. Sée concludes that this drug cannot be regarded as a specific or counter-poison in the various forms of malarial fever, as it does not prevent malarial poisoning when taken as a prophylactic; it does not prevent recurrence after a variable period, and it is useless in some of the most fatal forms, especially where the fever tends to assume a continued type. He ascribes its effect in ague to its action on the heart, diminishing its frequency and force; to its action on the peripheral arteries, lowering their tension and producing dilatation; to its action on the spinal cord and vasomotor centres, acting as a sedative and diminishing their excitability; and, lastly, to its cooling action on the system generally. He considers it the most valuable remedy in acute rheumatism, and here again he sees in its physiological action the most precise indications for its use. He gives it in doses of from ½ to 1½ grammes daily, increasing it if needful to 2 or 3 grammes. It is only in the acute stages that M. Sée considers it beneficial, except for the relief of pain, and in this respect it is also useful in gout.

THE PHYSIOLOGICAL AND THERAPEUTICAL ACTIONS OF SAFFRON.—Dr de Savignac attributes stimulant and sedative properties to saffron. In dyspepsia, flatulence, enteralgia, etc., it acts beneficially as a tonic, stomachic, carminative, and sedative. He also accredits it with emmenagogue properties; he thinks it possesses properties like those of castoreum, assafœtida, and valerian in controlling those disturbances of the nervous system which affect the uterine functions. Dr de Savignac considers saffron as a valuable medicament in the treatment of some diseases of women and children.—*Bull. Gén. de Thér.*, 15th and 30th May.

THE MEDICAL PROPERTIES OF SILICA IN CANCER, FIBROID TUMOURS, AND DIABETES.—A paper on this subject, from the pen of Dr R. Fawcett Battye, has appeared in the *Edinburgh Medical Journal* for November 1874.

SILICATE OF SODA IN VESICAL DISEASE.—Dr M. Dubreuil advocates the injection of silicate of soda to prevent decomposition of urine in cases of vesical disease. In a case of vesical disease from enlarged prostate he injected a solution of 4 grammes in 150 grammes of water, with the effect of restoring the urine to its normal condition.—*Gaz. Méd.* and *N. Y. Medical Journal*, Jan.

REMOVAL OF A FOREIGN BODY FROM THE EXTERNAL AUDITORY CANAL.—Mr Dalby related the method he had employed to extract some plaster-of-Paris which had become moulded and hardened in the external auditory meatus. The accident had occurred to a gentleman while he was having a cast taken of his head, and the plaster at that time had found its way into the meatus, and rapidly hardened. It partially filled the passage, was in close contact with the tympanic membrane, and caused complete deafness and considerable tinnitus. The following was the plan adopted: Whilst light was thrown through the speculum worn on the forehead, strong hydrochloric acid was applied on the end of a pointed piece of wood, drop by drop, to the centre of the mass, care being taken not to allow any of the acid to touch the meatus. Several drops of the acid were thus applied on four occasions, and the concrete became so far softened that a small hole could be drilled into it with a cataract needle, and pieces were from time to time picked out with a little hook, which is in common use among dentists. The great mass of the plaster, however, was softened by the application of the acetate of lead, the action of which was to convert the sulphate of lime into sulphate of lead and acetate of lime. The latter salt being very deliquescent, the consistence of the plaster was so altered that the whole of it was finally piecemeal removed with the hook referred to. Throughout the proceedings, which occupied eight sittings of about half an hour each, reflected daylight was employed. The tympanic membrane was found uninjured, and the hearing unimpaired.—*Clinical Society of London*, 9th Oct. 1874.

MR HENRY of Dublin describes, in the *Dublin Journal* for September 1874, a simple, safe, and effectual needle-guard for use in harelip and other plastic operations. It consists simply of two small plated tubes, each about three-quarters of an inch long, open at one end. These are united to each other by an elastic ring which keeps them in position on the ends of the needle.

SUPPURATION OF A GLAND DEEP IN THE NECK, IN A HOPELESS CASE OF CANCER OF THE TONGUE. INCISION.—Professor Broca lately had a case of this nature in his clinique. Four days after the first indication of pain and stiffness in the neck, the swelling was enormous, the voice was affected, and respiration was already impeded. Other glands were already hard and cancerous, but though there was no fluctuation, a very important sign of deep suppuration was present, *i.e.*, *swelling of an œdematous character with a rose-coloured tint of the skin.* This indicates deep-seated suppuration in a state of infiltration, if not actually collected as an abscess, and is an indication for deep incision. M. Broca made a long incision along the edge of the sternomastoid, without reaching pus, but eventually got it deeply seated by the use of a tunnelled sound, after which he inserted a drainage tube. The patient recovered from this attack, and six months later was still alive, though the cancer was making progress.—*Journal de Médecine et Chirurgie Pratiques*, Oct. 1874.

Dr Barbosa of Lisbon relates at considerable length the history of a case in which he tied the common iliac artery in a man æt. 62, for a large inguinal aneurism. The patient died after seven days, apparently from blood-poisoning and chronic peritonitis. The paper contains an account of other cases in which the vessel has been tied, and very careful clinical observations on pulse and temperature.

Part Fifth.

MEDICAL NEWS.

Perils of Medical Experts.—Dr Guichard of Troyes, some years ago, had reported on a case of rape, in consequence of which the criminal, named Lebeuf, was condemned. He vowed a mortal enmity against Dr Guichard and his colleague at the trial, Dr Carteron, and a few days ago went to Dr Guichard's house and fired two shots at him with a revolver, one taking effect on his forehead, the other near the heart. He died in about twenty minutes. When the assassin entered the doctor's room, he was sitting with his child of a few months old on his knee. The murderer has been arrested.—*Revue de Thérapeutique Medico-Chirurgicale*, No. 21, 30th October 1874.

Blood-Drinkers.—Upon inquiry at slaughter-houses, it is found that there are nearly two hundred persons in the city of New York who are in the habit of drinking blood flowing warmly from oxen, for strengthening purposes and for the cure of certain diseases. A lady is reported to have spoken to an inquirer as follows: "Professor Velpeau, of Paris, prescribed blood for me. I was consumptive and hastening to the grave. It has prolonged my life fifteen years. I had the utmost repugnance for it at first, but now a half-pint of hot blood from a well-conditioned ox is the greatest luxury of my life. My sister's baby so far has been preserved and nourished with little else but blood. I know twenty persons who drink it in my own neighbourhood to whom I have recommended it. It has extraordinary effects on some people, especially women, but should not be resorted to unless there is absolute weakness of the system." On a visit of the inquirer to a slaughter-house in Tenth Avenue, near Forty-second Street, he found a delicate-looking woman with a sickly boy holding a glass to the blood which ran from an ox with his throat cut. Both drank two or three glasses in turn, and departed with an appearance of added vigour. One of the butchers was asked whether he ever drank blood, and is stated to have replied to the following effect: "Sure an' I do, now; why not, now? ' Faith, an' ye couldn't tell the difference between it an' milk. It's just as swate, shure, and in the winter it's warm and foine. Bedad but it's stringthenin', sure. Hould on, an' I'll get ye a drap. It's best warrum—runnin' right from the baste." The proprietor said, "All last winter we had men, women, and children every morning to drink blood. They always imbibe beast's blood; never the blood of sheep. Some of them wince a bit at first, but, when you close your eyes, blood warm from the beast's neck has just the same taste as warm milk from the cow. We don't charge for the blood, excepting when we sell it to sugar-refiners." The blood of beeves is asserted to be more efficacious for weak lungs than cod-liver oil.—*Philadelphia Medical Times*, 3d Oct. 1874.

OBITUARY.

Sir J. Ranald Martin, C.B., F.R.S.—This eminent soldier, physician, and sanitary reformer, may be said to have died in harness; for though, when he resigned his seat at the India Board a few weeks ago, it was said that he was still in good health, he has not long survived to enjoy his well-deserved leisure. Like many other distinguished Scotsmen, the son of a minister in Skye, he worked his way up in the Indian Medical Service by hard fighting and diligent study of disease in all its most dangerous forms, till, on the subject of Diseases of Tropical Climates, he was an undoubted and unimpeachable authority. Leaving the service in 1840, after having occupied one of the highest posts in it, he settled in London, where, as an authority on tropical diseases, he soon attained a very large practice among Anglo-Indians. He also did much valuable service in the India Board, and in advancing the interests of the Navy and Army medical services in every way in his power.

Mr John B. Foster.—By the sad and sudden death of this gentleman, the profession in London has lost one of its most promising members. A distinguished student of University College, and a most industrious dresser in its hospital wards, he attracted the notice of Sir Henry Thompson, and for nearly ten years he has acted as his trusted assistant and friend, taking the sole charge of his practice in the autumn months. He died in his thirty-third year, on Wednesday, 17th December, of meningitis, which had come on on the fourth day after an extensive accidental burn of his face, neck, and hands, the result of an explosion of gas in a patient's house.

TO CORRESPONDENTS.

No notice can be taken of anonymous communications. Whatever is intended for insertion must be authenticated by the name and address of the writer—not necessarily for publication, but as a guarantee of good faith.

PUBLICATIONS RECEIVED.

William Aitken, M.D.,—Outlines of the Science and Practice of Medicine. London, 1874.

Dr M'Call Anderson,—Practical Treatise on Eczema. London, 1874.

J. Solis Cohen, M.D.,—Croup, in its Relations to Tracheotomy. Philadelphia, 1874.

J. M. Da Costa, M.D.,—On Strain and Overaction of the Heart. Washington, 1874.

Edward Long Fox, M.D.,—Pathological Anatomy of the Nervous Centres. London, 1874.

Heinrich Frey,—Histology and Histochemistry of Man, translated by Arthur E. J. Barker. London, 1874.

Henry Hartshorne, M.D.,—Essentials of the Principles and Practice of Medicine. Philadelphia, 1874.

J. A. B. Horton, M.D.,—Diseases of Tropical Climates and their Treatment. London, 1874.

Joseph W. Howe, M.D.,—The Breath. New York, 1874.

C. F. Maunder,—Tumour of Lower Jaw Removed without External Wound. London, 1874.

Dr Auge Monteverdi,—Note sur un Moyen de distinguer la Mort Vraie de la Mort Apparente de l'Homme. Cremone, 1874.

Nordiskt Medicinskt Arkiv. Stockholm, 1874.

Edmund Owen,—Introductory Address to the Students of St Mary's Hospital, Oct. 1, 1874. London, 1874.

J. Russell Reynolds, M.D.,—Address in Medicine delivered at the Meeting of the British Medical Association. London, 1874.

Sydney Ringer, M.D.,—Handbook of Therapeutics. London, 1874.

S. James A. Salter,—Dental Pathology and Surgery. London, 1874.

Steiner's Compendium of Children's Diseases, translated by Lawson Tait, F.R.C.S. London, 1874.

Transactions of the Clinical Society of London, vol. vii. London, 1874.

Transactions of the Pathological Society of London. London, 1874.

Prof. Von Tröltsch,—Surgical Diseases of the Ear, translated by James Hinton. London, 1874.

Twenty-third Report of Lunatic Asylums in Ireland. Dublin, 1874.

C. Dott. Vivante,—Della Cura Lattea. Venice, 1874.

William Williams,—Principles and Practice of Veterinary Medicine. Edinburgh, 1874.

PERIODICALS RECEIVED.

American Journal of Insanity,—July, Oct.

American Journal of the Medical Sciences,—October.

American Journal of Obstetrics—Nov.

American Journal of Syphilography and Dermatology,—October.

American Practitioner,—Aug. to Nov.

Annales d'Oculistique,—July to October.

Archives Générales de Médecine,—Sept. to December.

Archives de Physiologie,—July, September.

Archives de Tocologie,—Sept. to Dec.

Australasian Medical and Surgical Review, July to September.

Berliner Klin. Wochenschrift,—August 31 to December 21.

Births, Deaths, and Marriages, Monthly Return of,—August to November; Quarterly Return,—September.

British and Foreign Medico-Chirurgical Review,—October.

British Med. Journal,—Aug. 29 to Dec. 19.

Bulletin Général de Thérapeutique,—Sept. 15 to December 15.

Chicago Journal,—July.

Detroit Peninsular Journal of Medicine,—November.

Detroit Review of Medicine,—September, October, December.

Druggist,—July to September.

Dublin Jour. of Med. Science,—Sept. to Dec.

Echo de la Presse Médicale,—October 8 to December 22.

France Médicale,—August 26 to Dec. 16.

Gazette des Hôpitaux,—Aug. 22 to Dec. 22.

Gazette Hebdomadaire de Médecine,—Aug. 28 to Dec. 18.

Gazette Médicale de Paris,—August 29 to December 19.

Guadalajara Repertorio Jalisciense de Medicina y Cirugia Practicas,—Nos. 1, 2.

Indian Medical Gazette,—Aug. to Nov.

Journal de Médecine et de Chirurgie,—Sept. to December.

Journal de Thérapeutique,—August 25 to Dec. 10.

Journal of Mental Science,—October.

Kin-Si I-Setzu (Japanese Modern Medical News),—No. 2.

Klinische Monatsbläter für Augenheilkunde,—April, July.

London Medical Record,—Aug. 26 to Dec. 23.

Lyon Médical,—August 30 to December 20.

Medical Press and Circular,—August 26 to December 23.

Med. Times and Gaz.,—Aug. 29 to Dec. 19.

Missouri Clinical Record,—August, Sept.

New York Medical Journal,—Sept. to Dec.

Obstetrical Journal,—Sept. to Dec.

Philadelphia Medical and Surgical Reporter,—August 15 to December 5.

Philadelphia Medical Times,—August 8 to December 12.

Practitioner,—Sept., Oct., Dec.

Progrès Médical,—August 29 to Dec. 19.

Psychological and Medico-Legal Journal,—August to November.

Public Health,—September 16.

Revista Medica de Chile,—October.

Revue de Thérapeutique Médico-Chirurgicale,—September 1 to December 15.

Sanitarian,—December.

Sanitary Record,—Aug. 29 to Dec. 26.

Virchow's Archiv,—April 10 to Nov. 11.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Suppurating Ovarian Cysts.* By THOMAS KEITH, F.R.C.S.E.

THE following narratives help to show, that operation ought to be the rule of practice in cases of acute suppurating cysts, or when typhoid symptoms come on after tapping.

Ten years ago, when cases of ovariotomy were few, and there was little to guide one in unusual circumstances, a young woman in the last stage of ovarian disease came to me a long journey from the north. The fatigue of travelling was too much for the strength that was left, and she arrived completely worn out. It did not seem possible, that in such a condition life could be prolonged many days, for the pulse was almost imperceptible, there was vomiting and diarrhœa, œdematous limbs, and albuminous urine, while a profuse fœtid discharge was going on from an opening near the umbilicus. The intensity of this putridity was such, that one became aware of it before entering the house, and the antiseptics of those days were powerless to arrest it. Day after day, I went expecting and hoping to find her dead, yet, though shrivelled up like a mummy, with an aspect scarcely human, respiration went on for nearly a month, the brain retaining its clearness, acutely alive to what was going on around. To remove a putrid cyst in such a condition of feebleness did not at that time even occur to me; yet since then, I have operated more than once, under circumstances not less unfavourable, and, looking back upon this case now, I think that operation might have turned out well; certainly death after it would have been the more merciful way.

Soon again (December 1864) there came another case of very large tumour. The patient had been jolted for some hours in a coach, and in the hope of relieving the pain thus set up, tapping was performed after her arrival. The pain was not relieved, abdominal distention from flatus became excessive, and typhoid symptoms rapidly set in. Fearing a repetition of the slow death process—which those who saw will not easily forget—ovariotomy was this time performed during the semi-delirium of septic fever.

This was probably the first time that surgery broke in upon an acutely inflamed peritoneum. The intense lividity, amounting almost to blackness, of the abdominal contents, and the spongy tenderness of inflamed intestine, were then strange to me, though thought little of now. Recent lymph was present everywhere, adherent bowel and mesentery hedged in a thick-walled cyst, the base of which was in a complete state of slough. Inflammation had gone on to gangrene, and there was intense putridity, just as in the previous case. After an operation which went on for two hours, the patient was placed in bed, cold, vomiting, and nearly pulseless. It seemed as if we had simply killed her, yet she got rapidly into heat, the restless delirium at once disappeared, there were warm perspirations, much sleep, and a recovery without a drawback.

This case, which was at the time fully reported in the *Lancet*, 1865, p. 480, has been to me as a landmark. Since then I have ten times met with cases of acute suppurating cyst, besides two chronic cases. In all of these, save one, the chance of ovariotomy was given, however hopeless-looking the case might be. In the exceptional case, ovariotomy would also have been performed, had it been possible to remove the patient from her poor home and unfavourable surroundings. She was seen with Dr Menzies on the third day after her fourth confinement. He had been called to her for the first time only the day before. A large ovarian cyst had existed with at least two of her pregnancies. The distention was so enormous that urgent dyspnœa had to be relieved at once by tapping. Upwards of six gallons of fluid containing much blood and pus were got away, and ovariotomy was agreed on as soon as she could bear removal. This could not be accomplished, and after three weeks, tapping was again had recourse to. This time the pus was intensely putrid, and as the canula got choked with pieces of fœtid lymph, an incision sufficient to admit two fingers was made into the cyst, and its putrid contents thoroughly cleared out. Fortunately the cyst was single; a perfect recovery took place, and this patient has had two children since. None but the strongest of women could have borne the exhausting suppuration that went on for nearly four months. Pulse and temperature remained high, and of at least six weeks of her illness she has now almost no remembrance. Recovery in such circumstances must be rare; yet it may be well to note, that during the whole time she was supported entirely on milk and butter-milk, and had no stimulants whatever; neither was there any washing out of the cyst.

Of the ten more or less acute cases operated on, eight recovered, while the two chronic cases got well easily. During 1872–3 several came about the same time, and the following series of seven occurred in the course of my second hundred operations for ovarian tumour, none of which have yet been published. To an onlooker, few operations look so hopeless as those for the removal of acute suppurating cysts. The general condition is always unfavourable, and as a rule ovariotomy is in these circumstances tedious and severe. To be believed in, such cases need almost to be seen.

Suppurating Ovarian Cyst. Ovariotomy. Recovery.

Mrs M., aged 35, was sent to me in the end of June 1871 by Dr Soutar of Golspie. An ovarian tumour was detected towards the end of 1869. In January 1870, she had severe abdominal pain. After a fortnight's rest, this passed off, but only to return with increased severity. Loss of flesh and rapid growth of the tumour followed, and it was nearly a whole year ere she was again able to be out of bed. During this time her sufferings, as told by a friend, must have been great. Often for weeks together she could not be moved from one position, while the changing of her dress, or the arranging even of the bed-clothes, brought on such pain that her cries were heard in the street. It was eighteen months after her first illness that she was able to make the journey to town. I saw her after she had rested two days. The pulse was then 156; the temperature 103°.

She was a tall, fair-complexioned, blanched-looking woman, extremely emaciated; the lips and fauces were very anæmic; the girth at the umbilicus was 46 inches; the lower part of the tumour felt solid, but fluctuation was distinct above the umbilicus; the abdominal wall was hard, thickened, and œdematous; the skin even in some places feeling as if adherent. It was evident that there were adhesions of a very unusual nature.

Two days after this examination, with the assistance of Dr Drummond of Nice, I removed three gallons of thick pus by tapping some inches above the umbilicus. A large, prominent, hard tumour remained below this. Much relief followed, and for a few days the pulse and temperature somewhat fell. In three weeks the cyst had refilled; the pulse was again rapid and feeble, varying from 120 to 160; the morning temperature was 101° to 102°; that of the evening 103° to 104°, sometimes higher. The skin was dry and shrivelled, and she was, if possible, thinner than before.

Ovariotomy was performed on the 13th of July 1871. Sulphuric ether was given. The incision extended from the umbilicus downwards eight inches. The wall was much thickened, the peritoneum of almost cartilaginous hardness, and the whole parts so unusually vascular, that no time had to be lost in completing the operation. The upper cyst was emptied of its purulent contents, the lower semi-solid portion thoroughly broken down, and the cyst walls, weighing 18 lbs., dragged out. There was not any part of the tumour non-adherent. The connexions were of the utmost firmness, especially those in the pelvis. Posteriorly, there was more adherent intestine and mesentery than I have met with except twice. The peritoneum was thickened by old lymph. Large flakes, like pieces of cartilage, were peeled off the wall after removal of the tumour. Some of these were as large as the hand, and it was difficult to tell what really was the peritoneum. All bleeding points were tied with Lister's ligatures, a broad thick pedicle secured by a clamp, and the wound closed with silk sutures.

The operation lasted upwards of an hour; much blood had been lost, and she was placed in bed with great fears for her immediate safety. She lay for some hours with an almost imperceptible pulse. She was restless, and great bursts of clammy perspiration broke out every now and then, such as one sees in those suffering from the shock of injury. Fortunately, there was no vomiting. By evening she was comfortably warm; flatulence was troublesome; there was much thirst. Pulse, 125; respirations, 32; temperature, 102°.

She slept during the night, but got low and faint towards morning, and there was some vomiting. Brandy and soup enemata were given every two or three hours. She improved towards evening. Flatus first passed forty-four hours after operation. The pulse was rapid and feeble, and she scarcely opened her lips for many days. In the third week there was pain and swelling in the right iliac fossa, and fluid formed. Four weeks after operation, this swelling was punctured, and about a teacupful of yellow serum was removed by a syringe; the rest was absorbed. She was able to return home in five weeks, and is now a strong healthy woman.

Acute Suppurating Cyst. Ovariotomy. Recovery.

In May 1872, I met at Newbigging, Dr Thomson of Inveresk and Dr Scott of Musselburgh, to consider the question of ovariotomy in the case of one of their patients, 40 years of age, with a large adherent multilocular tumour. She had been a healthy woman, had worked on till she could work no longer, and was now confined to bed. The umbilical measurement was 43 inches, and the cyst was very tense. The uterus lay in the mouth of the vagina, and the pelvis was filled with tumour to within an inch of the anus. Under these circumstances a preliminary tapping was recommended, and a pailful of very thick fluid was removed from the largest cyst, leaving the abdomen of considerable size. The relations of the uterus and pelvic portion of the tumour remained as before.

She recovered well, and remained so for two months. She had then shivering fits from time to time, and there was pain and tenderness in the abdomen. When I saw her now, she was much emaciated, and was losing flesh every day. She was flushed, and looked extremely ill; the tongue was dry and red, the pulse 120, temperature 102°. The tumour was as large as before the tapping; operation was advised without delay.

This was done on the 27th July; Dr Thomson, Dr Scott, and a number of visitors were present; sulphuric ether was given. The anterior cyst was full of thick pus. It was emptied, and the tumour thoroughly broken up through it, before interfering with the adhesions. These were extensive and firm; consisting of parietal adhesion extending over the whole anterior surface of the cyst; also omental, intestinal, and mesenteric. The tumour was generally adherent in the pelvis, to the bottom of Douglas's space, and to the rectum. The pedicle was long, and as it was adherent and a good deal torn, it was divided by the cautery close to the uterus. The operation was a severe one, and lasted an hour and a half. The weight of the broken-down tumour and contents was 30 lbs.

There was more pain than usual, and two opiate enemas were given in the afternoon. By evening she was in a drenching perspiration; pulse 125, temperature 100·6°.

She had not a good night on account of pain. In the morning the pulse was 130; perspiration was very free. During the day opium was again necessary to allay pain and quiet restlessness. By evening the pulse had risen to 140. By midnight there was fulness of abdomen, and the pulse had risen to 150-160, and for some hours it could scarcely be counted. Free stimulation was necessary. Flatus began to pass freely, and towards morning she slept well. Convalescence after this was quite satisfactory. She has enjoyed excellent health since.

Acute Suppurating Cyst, accompanied by very low Temperature. Recovery after Ovariotomy.

In the end of August 1872, Miss H., aged 57, came to me from Mr Welford of Sunderland. She had a large multilocular tumour filling the abdomen, and had been tapped two months before by Dr Ramskill of Staindrop.

She was pale, sallow, of not very healthy aspect, and the pulse was feeble. On account of œdema of the limbs and abdominal wall she was again tapped a few days after her arrival. The size of the abdomen was reduced one-half by this. The fluid removed was dark, thick, and viscid, but as it presented no unusual appearances no particular examination was made. In a few days the œdema disappeared, and she was able to be out of bed, but she did not gain strength. There was a return almost immediately of the swelling of the limbs, while her feebleness and unhealthy aspect increased. The urine was slightly albuminous; then pain and tenderness came on in the left side under the ribs, and she rapidly lost flesh. The pulse rose from 100 to 110-120, and it was now observed that the temperature was always under the normal standard. Altogether the case looked very like one of malignant disease either in the abdomen or somewhere else, and I thought of giving her one more tapping and then sending her home. She seemed far too feeble for a severe operation. The abdomen was now larger than before, measuring nearly 46 inches, and the œdema of the legs, vulva, and abdominal wall very great. Much to my surprise, two gallons of horribly putrid pus, with a gangrenous odour, were drawn off. From the extreme lowness of temperature, then 96·5°, I did not suspect that suppuration of the cyst could exist.

The presence of pus changed at once the aspect of the case and the prospects of the patient. The nurse, who had been let go on her holiday, was brought back after some days' delay. For two days after the tapping there was a little relief; then the pulse continued to rise and the temperature day by day to fall. Dr Foulis saw the patient with me. Several thermometers were used, so that there was no mistake in the observations as to the extreme lowness of the temperature. Her condition was now remarkable. The tongue was dry and red, and there was diarrhœa. She daily acquired more and more the look so characteristic of the last days of ovarian disease. The eye was dull and vacant; she lay all day, wiping with a towel the perspiration off her head and neck, indifferent to everything around. She took no food unless it was pressed upon her, and a "yes" or "no" was all that could be got out of a naturally lively woman.

Ovariotomy was performed on the 21st September with the assistance of Dr Sidey and Dr M'Gibbon. Sulphuric ether was given by Dr Foulis. The hope of saving life was indeed feeble. The emaciation of the trunk was extreme; the lower limbs were œdematous. The heart's action was so feeble, that on the morning of the operation the pulse could scarcely be counted at 130. The respirations were 26. In the axilla the temperature was 95·5°, in the mouth 95°. There was no mistake as to these observations. The wall was so œdematous that the serum spirted up on making the incision through the skin. The large cyst was emptied, opened to admit the hand, and the large semi-solid portion broken down. The whole was in a state of suppuration, and the putridity was horrible. The opening in the cyst was then closed by several pairs of forceps, and held forwards. The hands were carefully washed before separating extensive and firm adhesion from the whole wall. Posteriorly, there were numerous loops of adherent intestine and much mesenteric adhesion. Those of the mesentery bled freely, and at least twenty bleeding points were secured by catgut ligatures. The pedicle was divided by the cautery and returned. The uterus and left ovary were healthy. There was an unusual amount of oozing on account of the œdematous condition of the soft parts, and the wound was not closed for an hour and a half, when the sponges came out almost dry. The broken-down cyst walls weighed 12 lbs. besides the contents.

She looked almost dead when placed in a warm bed. Brandy was injected into the rectum, and the treatment was the same as in the previous cases. She did not awake from the sleep of ether for two hours and a half. She was then warm, not restless; the pulse was 170, but quite distinct, and stronger than in the morning.

At 10 P.M. she was in a free warm perspiration, and had slept two hours. The temperature in the mouth was 96°, in the axilla 97°; pulse 144.

She had much sleep during the night, and next morning the look of suffering was already gone. Flatus passed freely twenty-two hours after operation. She now took food and stimulants well.

The second day the pulse continued at 140, and the temperature at night had risen to 100·4°. On the third day it had fallen to 98·2°, and remained so. By the tenth day, the pulse had come down to 100. She went home to Staindrop four weeks after, and has had excellent health ever since.

Acute Suppurating Cyst. Ovariotomy. Recovery.

On the 15th December 1872 I saw a young Canadian lady in her twentieth year, with an ovarian tumour of rapid growth. She was sent by Dr Campbell and Dr Drake, of Montreal. She had lost a sister a few years before from ovarian disease after repeated tappings. Yet, in the present case, there was nothing to cause alarm till the beginning of October. The umbilical measurement was then 31 inches. Three weeks before leaving Montreal she had a sharp peritonitic attack. The fatigues of the voyage and the journey to town were well borne, but the drive from the railway to her lodgings brought on severe pain. Being then from home, I did not see her for a fortnight. During all this time the pain continued, and she was confined to bed. Dr Parker, of Halifax, an old friend of the family, was fortunately in town. He took charge of her till my return, and continued to give me his kind assistance and counsel in the after management of an unusually anxious case.

By this time the general condition was unfavourable, and it was almost certain that suppuration was going on in the cyst. The pulse kept about 120; the morning temperature was 101°, rising towards evening. The abdomen measured 37¼ inches. It was tense and tender, the tension being most marked in the epigastrium, where there was a large cyst. Hoping to give some relief and gain time, I punctured this cyst some distance above the umbilicus. The trocar used was a large one, but only two pints of thick fluid, containing some pus mixed with plugs of fibrine, were got away, though the instrument could be moved in a large cavity. Fortunately the adhesion existed at the point selected, and no harm was done by the puncture. Tension was slightly diminished, but no improvement in the general state followed; yet she was able in ten days to be carried into a more airy lodging, and ovariotomy was performed on the last day of 1872. The pulse was then 120; temperature, 100·5°. Her condition was far from good, but there was no chance of her getting into a better.

The largest cyst was opened, and its purulent contents allowed to escape. The lower half of the tumour was made up of small cysts, mostly in a state of suppuration. These were carefully broken down, for the whole was very friable, as all rapidly growing ovarian tumours are, and more than ordinary care was needed to hinder the contents from escaping into the peritoneal cavity. There was general parietal adhesion, also to some folds of intestine and mesentery. In the pelvis the parietal adhesion was continued on behind the pubes to the utero-vesical pouch, and into the pelvis to the bottom of Douglas's space. There was considerable loss of blood, and oozing from the pelvic attachments went on for a long time. A small reflector was of much use in bringing some deeply-seated bleeding points into view, which, but for this, could not have been secured. The pedicle was on the right side; it was short, and was divided by the cautery. The left ovary and uterus were perfectly healthy. The operation went on for an hour and a half.

The pulse was very feeble when she was put to bed. Two hours after, when she first awoke from the ether sleep, it was 160, and the respirations 56. By evening the skin was sometimes hot, at other times there was free perspiration. She was extremely pale and feeble; flatulence was troublesome, and sickness was threatened from time to time. Pulse 130, temperature 100°.

1*st Jan.*—Quiet night; very pale; complains of being tired. Pulse 140, temperature 100·3°, respirations 30. Urine, six ounces, loaded with lithates. Flatus passed twenty-six hours after operation. Towards afternoon there was some bilious vomiting. The evening was fairly quiet; no perspiration; some fulness of epigastrium. Pulse 128, temperature 99·3°, respirations 28. Brandy and soup enemas had been given all day every two or three hours.

2*d.*—Bad night from vomiting; looks pale and ill, and is very nervous; some fulness of epigastrium, but flatus passes freely by tube. Pulse 125, temperature 100°.

3*d.*—Disturbed night from vomiting. Epigastric fulness increased. Temperature 101·8°. Vomiting continued at intervals during the day. Looks extremely pale and ill.

4*th.*—Better night. Tongue dry and red; much flatus coming up. No fluid could be detected in Douglas's space. In the evening the temperature was 102·4°, pulse 122.

5*th.*—Good night; abdomen hollow; all sutures suppurating; abdominal muscles are thrown into violent spasms when the dressing of the wound is changed. Cannot detect any collection of fluid, and Douglas's space is quite empty.

7*th.*—Some vomiting towards morning; abdomen generally distended; wound gray-looking; removed lower stitches and separated edges of lower part of incision. Temperature 101·5°, pulse 130, respirations 30.

9*th.*—Some dirty red serum escaping from lower angle of wound.

12*th.*—Discharge during last three days has been very free; now purulent, with large plugs of lymph. Drainage tube passes six inches. The collection seems to be abdominal, for pressure over right lumbar and iliac regions causes discharge to escape freely.

29th.—Very free discharge for a fortnight; wound now cicatrized.

A rapid convalescence followed, and since her return to Canada her health has been excellent.

Suppurating Cyst. Pyæmic Fever. Death from Septicæmia three days after operation.

Mrs W., aged 53, from Rose Street, was first seen by me in 1873, along with Dr Black, on account of a moderate-sized soft ovarian cyst. She was a large, sallow, fat, flabby woman, of unhealthy aspect. She kept an eating-house, and was a large eater of animal food. Though at this early stage of the disease she rarely left her bed, yet there did not then seem to be any call for interference, and I advised that, when necessary, tapping should be performed.

This was done three times in the course of the summer. In November symptoms of acute inflammation of the cyst set in, and a pailful of pus was removed. She was again tapped in December, and was then so ill that it was not expected that she could live many days. The pulse was rapid and feeble; tongue dry and red. There was much pain and constant retching; the temperature sometimes as high as 105°. There was now much solid matter left after tapping, and the part of the tumour occupying the pelvis was extremely sensitive.

She was somewhat relieved after a sixth tapping, the contents of the cyst being purulent as before. She was now very anxious to have any chance that operation might offer, for death was preferable to her present life. I had all along thought most unfavourably of her as a subject for ovariotomy, though I did not even now feel justified in drawing the line and refusing her the small chance that seemed yet left to her. She had spent the preceding eight months in a small back room, lighted only by gas, and had been in bed all the time. She was removed on the evening of the 23d of January into an airy lodging, a proper nurse was got for her, and ovariotomy was performed on the following day. Dr Parker of Nova Scotia, Dr Black, Dr Angus Macdonald, etc., were present. The pulse was then 120, and the temperature 101·8°.

The incision extended from two inches above to seven inches below the umbilicus. There were firm and general parietal adhesions, to the omentum, to a loop of intestine, to the whole under surface of the liver and gall-bladder. The anterior cyst was full of pus. The semi-solid portion was broken up in the usual way. The pedicle was unusually broad and short. It was divided by the cautery. Two vessels bled on removing the clamp, and were secured by ligature. The hæmorrhage from the under surface of the liver went on for some time. Pressure was kept up by sponges, till a thin coagulum formed, which was allowed to remain. In consequence of this, there was much exposure of the upper part of the abdominal cavity.

The operation lasted upwards of an hour. There was no sickness, and in the afternoon perspiration was free. At 10 P.M. her condition was most satisfactory. The pulse had fallen from 120 before operation to 106. The temperature, which was then 101·8°, was now 99°.

She had a quiet night till 5 A.M., when she awoke with severe catching pain over the liver, with flatulence, and a tendency to sickness. At 9 A.M. the pulse had risen to 118. The temperature was 99°. She had a dusky gray look. Vomiting came on in the course of the day, and was severe towards evening. Perspiration had ceased, and there were only a few ounces of urine, loaded with lithates. The pulse had risen to 145; temperature, 100·3°.

The second night was quiet, and there was no more vomiting. Urine was also clear and more abundant. There was no distention, and no movement whatever of intestines. The pulse got more and more feeble, and she died towards the end of the third day.

The abdomen was flat; the epigastric hollow quite distinct. The wound was healed, and the omentum was slightly adherent to the wall. On separating this downwards, a cavity, containing eight ounces of red serum, surrounding the pedicle, was opened into. The stump of the pedicle was as it had been left, and was vascular to the edge. In the pelvis there was just a trace of clear yellow serum. The small intestine under the omentum was quite pale, and there were no traces of general peritonitis. On the under surface of the liver, where the worst adhesion had been at the operation, there was about one ounce of pus and soft thick lymph.

Acute Suppurating Cyst. Rupture into Peritoneal Cavity. Recovery after Ovariotomy.

F. H., aged 27, from Cockenzie, was brought to me in August 1872, on account of a multilocular ovarian tumour which had been noticed eighteen months before. She had for a long time suffered from chronic chorea; she was anæmic, had a dusky leaden complexion, and was of unhealthy aspect. The skin was dry and harsh. She never perspired, was withal fleshy, and had no teeth. A more unfavourable general condition for operation could hardly be imagined. From her agitation, no examination of the abdomen was possible without putting her under an anæsthetic. The tumour was then found to be non-adherent, reaching to near the ensiform cartilage. The uterus was scarcely movable, and its connexion with the cyst was evidently close.

Eight months after, April 1873, tapping was necessary, though her general condition had improved. The fluid removed contained much blood. For four months after she remained well, when in

September she had severe pain in the tumour, and other symptoms of inflamed cyst. Towards the end of September, a pailful of pure pus was removed, with much relief. She was sent to the country to recruit for a week or ten days. A few days after I was sent for to see her. She had felt ill ever since leaving town, and had got chilled on the way home; twenty-four hours before, she was seized with intense abdominal pain, and had been in a state of collapse ever since. When seen, she was almost pulseless, quite cold, with lividity of hands and feet, while the laboured catching respiration showed the nearness of approaching death. The abdomen, which a few days before was only of moderate size, was now enormously distended, and resonant at the umbilicus. It was evident that the cyst had given way, and that its purulent contents had escaped into the cavity of the peritoneum. Though it seemed then almost useless, I turned her on her side, opened the abdomen with a lancet at the umbilicus, and pressed out two basinfuls of pus and fibrinous clots. The wound was then closed with a pin. The distention was still great, though she expressed herself as much relieved. A number of hot bottles were put about her, and I left, doubtful if she would live through the night.

I heard next day, that after some hours she got warm, and in a fortnight she had so far recovered that it seemed right to give her the chance that ovariotomy offered, small though it seemed. She was now much reduced, and the skin was again dry and harsh. The pulse on the morning of operation—23d October—was 110, very feeble; the temperature was barely 97°.

Adhesions were almost universal, mostly recent, and bleeding was unusually free. The peritoneum was of a livid colour, showing everywhere signs of recent inflammation. The largest cyst was full of pus. The adhesions were most firm in the right iliac and lumbar regions, and the whole under surface of the liver and gall-bladder were adherent. As the broken-down tumour was being separated from its connexions in the pelvis, a collection of thick pus like putty was opened into. This occupied the right side of the pelvis as low as the bottom of Douglas's space, and without doubt consisted of the non-absorbed thick portion of the purulent fluid which had escaped from the burst cyst three weeks before. All this was carefully removed. The cavity was lined by a thick membrane, which was peeled off in pieces, leaving a freely bleeding surface in the pelvis. The omentum was divided into three portions and tied with thick catgut ligatures. Numerous bleeding points were also tied, but the oozing from the under surface of the liver was most troublesome. Pressure was kept up by sponges, and for a long time these came away heavy with blood. There could scarcely be said to be a pedicle, for the cyst arose close to the uterus. A cautery clamp was applied, and one vessel bled on its removal. This was secured by transfixion. The handling of the parts caused other two large vessels to give way. Finally, after two hours and a half, the wound was closed. A glass drainage-tube was left, passing down to the bottom of the pelvis behind the uterus.

She was cold and feeble on being put to bed. An opiate and brandy enema were given. In the afternoon there was much restlessness and severe vomiting for about an hour. The sponge over the tube was soaked in blood, and in the evening upwards of an ounce was removed from the pelvis by suction. There was no perspiration. Pulse 118; temperature 100·5°.

First day after operation.—Looks pale and ill; skin dry; urine scanty and loaded with lithates; pulse 118. One ounce and a half of dark red serum removed from tube. At 3 P.M. two ounces were got away by suction, and in the evening about an ounce. This last serum was scarcely so red. It coagulated on cooling, and was swarming with vibrios. This was thirty-two hours after operation. Vomiting has been severe since afternoon. Face now flushed; skin dry and hot; no perspiration whatever. Pulse 120; temperature 101°.

Second day.—Good night; no more vomiting; condition improved; skin still shrunken and dry; not a trace of perspiration. Got away scarcely three drachms of nearly pale serum, but very fœtid. In the afternoon about one drachm, and two drachms in the evening. Pulse 100; temperature 99·8°.

Third day.—Good night. As there was little more than a teaspoonful of clear serum from tube, it was removed—sixty-eight hours after operation. Flatus passed downwards now for the first time. Still no perspiration.

Fourth day.—Good night. Much improved. Pulse 96. A little clear serum escaping from wound.

Fifth day.—Looks sallow and ill. Pulse 115. Tube was again introduced, and upwards of an ounce and a half of very putrid red serum removed.

Sixth day.—Very good night. Skin moist for the first time.

Seventh day.—Fair night. Perspiration. Two ounces of dark-brown putrid pus removed by suction. Pulse 105; temperature 101°.

Ninth day.—Discharge still continues very putrid; about one ounce in twenty-four hours.

Sixteenth day.—Some small pieces of charred tissue came away in discharge.

Twenty-sixth day.—Wound cicatrized. The patient is quite well, all traces of chorea having disappeared. I have seen her lately, in perfect health.

Acute Suppurating Cyst. Ovariotomy. Recovery.

J. M'K., aged 36, from Coupar-Angus, was sent to me early in July 1873 by Dr Kidd of Alyth, who had seen her for the first time a few days before. She had a very large adherent multilocular tumour of two years' growth. The measurement at the umbilicus was 52 inches, and from the ensiform cartilage to the pubes 31 inches. She was anæmic and emaciated.

After this, she went to the country, and on the 20th of July I tapped her, with the assistance of Dr Wallace of Kirkliston. The abdominal walls and limbs had by this time become enormously œdematous. Upwards of four gallons of thick viscid fluid were removed. A large multilocular tumour still remained filling the abdomen.

She was much relieved, and for some weeks gained strength. Towards the end of August she was seized with severe pain all over the abdomen. In the beginning of September, I found her with all the symptoms of suppurating cyst—dry red tongue, quick pulse, and high temperature. She was at once brought into town for operation, but was so feeble and ill that tapping was again done, and upwards of forty pounds of pure pus were removed. The tumour had much increased, the epigastrium being distended with a mass of cysts extending under the sternum.

She was now much emaciated. The pulse was 118, extremely small and feeble. There was dulness, probably from œdema, at the base of both lungs. The temperature was seldom much above 100°, never below it. The urine was scanty, and loaded with lithates. After a few days' rest ovariotomy was performed on the 13th of September. Dr Wallace was present. The incision was seven inches, afterwards enlarged to nine inches. Adhesions were firm from the pubes to the ribs. The peritoneum was much thickened, and broad flakes of old lymph were stripped off. A short, broad, thick pedicle was divided by the cautery. The uterus and opposite ovary were healthy. The wound was kept open for half an hour till oozing had ceased. The weight of the cyst walls was fifteen pounds; and upwards of four gallons of pus were removed from the largest cyst a few days before.

The operation lasted upwards of an hour and a half, and, like the other cases, she was very weak for some hours. There was no sickness. By evening there was free perspiration; the pulse was 140; the temperature had risen to 101·8°. Cough was very troublesome for some days.

She recovered rapidly, and was out of bed on the fifteenth day. Feeling too well, she sat at a window and caught a severe pleurisy of the left side. On account of this, she was again confined to bed for upwards of three weeks, and though much reduced, she was able to return home six weeks after operation. She soon regained her usual health.

The two chronic cases are not given in detail, as neither presented anything unusual. There was no rise of pulse or temperature at the time of operation. In both, the general condition was fairly good. In one, a robust-looking young woman, there was general tenderness; and for nearly four months she kept her bed, being unable to walk without pain. In this case there was no adhesion whatever, but the interior of the cyst was lined by a thick layer of lymph. In the other, there had been a very severe inflammatory attack some months before operation, and the tumour had existed during at least one pregnancy. All symptoms of irritation had subsided when she came to town, and there was not even tenderness anywhere. Adhesions were firm and extensive; the cyst was filled with pus and masses of old fibrine. Both these were clamp cases, and both recovered well.

In all the acute cases, there was pain or tenderness somewhere on pressure. In all, the pulse was rapid and feeble; exhaustion and emaciation were great.

As a rule, there was increase of temperature to a very great degree. This varied from 100° to 102° in the morning, to 103° or even 105° by evening. In all cases where the temperature was very high, there was a fall of several degrees within a very few hours after operation.

But two cases, and these were the most hopeless-looking of the whole number, were remarkable on account of the extreme lowness of temperature. In the third case, the temperature at the time of operation was only 95·5°. This is the more unusual from the circumstance that the contents of the cyst were intensely putrid. For some days before operation, there was a steadily rising pulse, and a steadily falling temperature. In the sixth case, where a suppurating cyst had burst into the peritoneum, the temperature at the time the patient was operated on, was 97°. In both of these, the temperature rose within six hours after removal of the tumours; in one case from 95·5° to 97°; and in the other from 97° to 100·5°.

In one of the acute, and in one of the chronic cases, there was no adhesion. In the others, adhesions were of the gravest nature, and the operations were tedious and severe.

In five cases the pedicle was treated by the clamp; in five by the cautery. One death followed each method. Death took place about sixty hours after operation from blood-poisoning. Both were looked upon as most unfavourable subjects for ovariotomy. In one, the chances were guessed at as twenty to one against the patient; while in the other, some of my friends remonstrated with me as to doing the operation at all.

In only two, did typhoid symptoms come on quickly after tapping, and in one case, tapping was had recourse to in the hope of relieving pain set up by a long journey to town. Five had not been tapped, and in the others no symptoms of distress appeared within less than five weeks after it. The simple tapping had probably little or nothing to do with the after suppuration. The usually received opinion, therefore, that suppuration in ovarian cysts is almost always the consequence of some interference, requires modification. So does the statement that when there is pain or tenderness this generally arises from inflammation of the peritoneal surfaces; the reverse is oftener the case. Tenderness or pain, if long continued, is almost always a certain sign that irritation is going on in the tumour somewhere. Simple surface tenderness, causing surface adhesion, rarely lasts long; and the worst cases of adhesion met with have often no history, either of pain or tenderness.

Though the diagnosis of the advanced stage of a suppurating cyst is generally easy, yet the early symptoms may be most obscure. Two years ago, Dr William Murray of Newcastle sent me for operation, a patient with a large single non-adherent cyst, of not very rapid growth. She was in the full vigour of perfect womanhood, and I advised delay till her health or comfort were more interfered with. She was extremely urgent to have the tumour taken away, and somewhat reluctantly her request was agreed to. She was then visiting friends, and had been sight-seeing, and had got a little cold. It was finally arranged that she should return for operation after the period, which was due in a fortnight. Next morning she felt weak, and the temperature was above 100°. There was some cough and bronchial râles, but as she seemed to be suffering merely from a feverish cold, she was allowed to return next day to her home in Sunderland. There was then not the slightest complaint of uneasiness in the abdomen, no tension, no tenderness. She was not again seen by Dr Murray, and no circumstantial account was obtained of her subsequent illness; but she never left her bed after getting home, and died in a few weeks, almost immediately after a large amount of pus had been removed from the cyst.

The extreme feebleness of pulse, and depression of the vital powers, which all cases of acute suppurating cyst present, must not deter one from operating. Hopeless without interference, they are not the unfavourable cases for operation that they seem, or, judging from the small number of reported successful results, have hitherto been regarded.

ARTICLE II.—*The Morisonian Lectures, delivered before the Royal College of Physicians of Edinburgh: Session* 1874. By J. BATTY TUKE, F.R.C.P.E., F.R.S.E.

LECTURE IV. (*delivered 27th March* 1874.)

MR PRESIDENT,—I find that so much time has been occupied in the consideration of the anatomical and physiological part of my subject, and of the idio-encephalic conditions, as to necessitate a somewhat cursory review of the evolutional causes of insanity, that is to say, those which are concurrent with the changes in the development and decay of the body; and also of those conditions which are concurrent with diseases of organs remote from the brain. Were I to enter in detail upon those subjects, it would be impossible to lay before you fully that department to which I have directed special attention—the morbid histology of the brain—and which I am naturally desirous to illustrate. This will be done in the last two lectures.

As has been already said, the evolutional conditions of the body which concurrently affect the brain, require to be studied in close association with the last-named idio-encephalic condition—instability of the nervous system,—and for this reason, that the exciting cause of the insanity occurring at the climacterics (to use the term of the ancients) is so comparatively slight, so common to all mankind, that it almost presupposes an hereditary or congenital defect in the organs implicated.

It may be said that this is true of all forms of insanity—that a man who becomes insane must have been predisposed to the disease; but with this I cannot agree, any more than I could with the statement that the subject of, let us say, chronic catarrhal pneumonia, must have been predisposed to diseases of the respiratory system; or, to take a better instance, that all cases of Bright's disease are dependent on some instability of the constituents of the kidneys. We may grant this to be true of the gouty or contracting, but not of the acute inflammatory, form of these diseases: and in like manner, we have a right to refer insanity connected with the pubescent or climacteric period to nervous instability; whereas we should not be justified in assuming the same of a case of mania incident upon bodily disease or toxic influences. That weakness of constitution may be a factor in the production of all forms of degeneration, whether of the lungs, the kidneys, or the brain, is no doubt true; still, in certain of them, it is by no means an essential one, for they may result from causes non-inherent in the individual. I would not have given expression to this truism, were it not that so many mistakes, implicating the comfort and happiness of individuals and families, are made, in attaching the stigma of a tendency to so-called mental disease.

To the public, madness is madness—no attempt is made to differentiate between madnesses and madnesses: to have had an insane member in a family is to cause the world to regard with suspicion the general psychical health of every member, without regard to the cause or to the kind of the insanity in the afflicted individual. The manifest injustice of this is palpable to the medical profession, as its members know to what various causes and influences insanity is ascribable, apart from hereditary predisposition. Thus we may have, and constantly do have, insanities arising concurrently with morbid conditions of the body as mere accidents in the course of the case, in very much the same manner as delirium or convulsions, and probably due to a similar cause or causes, from which the patient recovers without any permanent influence on his psychical health. The utmost care should be taken, in forming and expressing an opinion, as we are so frequently asked to do, as to the heredity of insanity in a family, to obtain the fullest evidence as to the general pathology of the case or cases of the individuals who have been its subjects.

But, after all, why should we speak of insanity as a stigma on a family, any more than we should of cancer or tuberculosis? Doubtless it is a terrible disease, but in what is it a more terrible disease than consumption? Only in this, that it is a more inconvenient disease. Is it in itself more loathsome than cancer or a dozen other maladies to which flesh is heir? Is it less curable than the majority of the graver forms of disease? Here it certainly has the advantage; for, given two persons, the one with the preliminary symptoms of tubercular phthisis, and the other with the preliminary symptoms of acute mania, *cæteris paribus*, of which would we predicate the greater probability of recovery? I am sure there is not a physician here present who would not prognose in favour of the latter. Society has surrounded insanity with a false metaphysical glamour, with which is compounded a savour of the ludicrous that does not enter into its estimate of what it considers purely bodily disease.

The observation of ages has caused us to accept as an ætiological fact, that numerous and various degenerations commence contemporaneously with modification of nutrition at the evolutional periods of existence; and it might be fairly asked, Why should mental alienations be classed in accordance with their period of incidence any more than other morbid conditions? To this question it may be answered, that the morbid conditions occurring at these phases of which I now speak are of such a definite nature as almost, if not completely, to warrant their assumption as pathological entities; that, were the type of other morbid conditions modified in any great degree by age, it would be competent to classify them accordingly, and that the terminology of certain forms of degeneration is influenced by such circumstances. The periods of pubescence and adolescence, the first and second climacterics, and the degenerations of old age, must be regarded as immediate causes of insanity in those constitutionally prone to nervous instability, and also in those who may be placed under such accidental circumstances as may have a depressing effect on nervous energy. The changes in the nutrition of the system which take place at these periods concomitantly influence the psychical condition of the most stable, and in the unstable produce an increase or decrease of functional activity of the brain-cells, which, overstepping the limit of health, leads to the evolution of abnormal emotion, and consequent abnormal actuation, the latter in certain instances tending to aggravate the condition.

Dr Anstie lays great stress on the occurrence of convulsions during teething without apparent adequate cause, as a strong indication of the insane neurosis, which, as the child merges into boyhood, is further evidenced by a tendency to steal, lie, commit acts of cruelty, and manifest cowardice and meanness of disposition. This is one train of symptoms; but there is another, of which a precocious æstheticism is the leading characteristic. It is open to question how far the latter is associated with the tubercular diathesis; in my opinion, the association is close and intimate. Under either of these circumstances, self-pollution comes in as an adjuvant to render the boy nervously unstable. There are high authorities who hold very strong views as to the evil influence exercised on the organism by the dietetic system of the present day: they maintain that in districts where the diet of the child and boy is simple, where the influence of strong food is withheld, the psychical health is better and stronger than in cities where over-nutrition demands of the youth some outcome of over-excited function. The extreme athleticism of the present day is regarded as a vicarious emunctory of over-excited function, actually called for by a too nutritious diet. Although we may not be all at one with these authorities, it cannot be denied that views, founded as these are on comparative observation, are well worthy of the attention of the profession.

Trying as pubescence is to the predisposed boy, he has yet a harder trial to undergo at the period of adolescence. As the virile function asserts itself on the organism, heredity frequently asserts itself. The evil habit of pubescence may have been continued uninfluenced by the self-respect which ought in the man to have overcome it; over-exertion of the brain by study may have deteriorated the vesicular elements beyond their power of recuperation; or the altered and altering nutrition may specially excite to action the function of the cells of the hemispherical ganglia; each condition producing a peculiar train of symptoms. Thus, the masturbator becomes peculiar, suspicious, reserved, full of faint fears, and self-accusing; the student melancholic, suicidal, and misanthropical; and the sthenic subject evinces maniacal excitement, irritability, a desire for action—he walks, talks, drinks, smokes, must ever be doing something. But whatever may be the general symptoms of these three sets of patients, they have one common symptom—a perversion or increase of the sexual instinct—the masturbator thinks women are looking at him, that he influences women unconsciously to themselves; the melancholic believes himself impotent; and the sthenic would run amuck amongst women. It is the last class which so often develop dipsomania. Insanity at this period is said to be influenced by sex; it is stated to be of more frequent occurrence amongst young women, and that the symptoms generally partake in a great degree of the character of hysteria. This is certainly not my experience, which may be exceptional—nay, is, if compared with that of most writers on the subject. I grant the hysteria, but not the insanity. It is contended that the evolutional change is more physically marked in the girl than in the boy. I fail to see this as a reason. I view it conversely; that the *absence* of an emunctory in the boy is more likely to make him prone to nervous disturbance than the girl, who has a physical vent. I can only instance my experience that absolute insanity is more frequent in lads, whilst the modified insanity of hysteria is more common amongst girls.

Between the adolescent and the climacteric periods the constitution of the nervous, like the other systems, becomes established, and disturbance is not liable to occur, except from some accidental circumstance apart from evolution. But when the great climacteric is reached, we find a singularly well-marked form of insanity manifesting itself *without any adequate cause* apart from the change of life. In those who have abided by the laws which regulate health, in those who have met with the success and comfort which as a rule accrue from a well-spent youth and middle age, in those whose constitutional proclivities are not towards nervous degeneration, this period is overcome with but slight constitutional disturbance, the middle-aged man or woman simply lapsing into the man or woman of advanced years. But in converse cases, the trophesial influence which grays the head, bows the back, flattens the foot, and modifies the general nutrition, concurrently reduces the vigour of the cerebral constituents. No more definite reason can be assigned for the one than can be for the other series of conditions. It is simply a law of nature that degeneration takes place at certain periods of existence; and as no degenerative change acts equally on all the systems, so each is liable to a predominance of decay, according to circumstances in the individual.

The influence of sex over the frequency of incidence of insanity at the climacteric period is slight. The following is the result of the analysis of 210 recent cases admitted into the Fife and Kinross Asylum:—Of these, 48 presented the well-marked train of symptoms of climacteric insanity—19 out of 86 men, or 22 per cent., and 29 out of 124 women, or 23 per cent. Before commenting further on this point, it may be well to state what these symptoms are. With slight modifications their leading characteristics are a vague fear of impending but undefined evil, a belief that the soul is for ever lost, that the patient has done some wrong to his family, that he is accused of some crime the nature of which he cannot determine, that he or his family is ruined, a strong tendency to suicide for the direct purpose of evading his misery, and, in a large majority of cases, an acknowledgment and recognition on the part of the patient of his own insanity. These are the leading symptoms, modified by idiosyncrasy. The general tone of melancholy pervading the condition may be influenced in the individuals by temperament, as every disease is. Melancholy may be characterized by excitement verging on mania, but still it is a melancholy mania. But what explanation can be offered of the fact that the incidence of these trains of mental symptoms at the climacteric period is equal, or nearly equal, in men and women? In women we have the cessation of a physiological emunctory, the catamenia, to assert as a cause. [But is there not, in man, a cessation, or at least a considerable modification, about this period, of an emunctory? Time will not serve to discuss this point—it is merely thrown out as a suggestion.]

It is unnecessary to do more than allude to senile insanity, as its general pathology is universally admitted. I see that Bucknill and Tuke include it in the idio-encephalic conditions, and I believe I did so myself in a paper written some years ago; but, on the whole, it seems to me now more pathologically correct to refer it to an evolutional condition, which is by no means limited to the nervous system; in fact, that to the morbid condition of the bloodvessels must be ascribed the degeneration of cellular activity and of brain tissue.

Nor will I dwell longer on those other evolutional conditions—the pregnant and the puerperal—interesting and important though they are, than will suffice to discuss the question of their genesis. It has been, and even now is, the custom to speak of the insanities which occur concurrently with pregnancy, follow parturition, and supervene on lactation, under the common designation of puerperal insanity. How erroneous this is must be evident, when we consider how different are the efficient causes and the mental manifestations of these three forms of insanity. The various and profound changes of the whole system which attend pregnancy do not leave the nervous centres unaffected. The most frequent indication of insanity in the fecund woman, is a mere exacerbation of the morbid longings or appetites, and the change of disposition and temper almost invariably accompanying pregnancy. These in the nervously stable subject do not pass beyond a slight and temporary alteration in the morale, which, slight and temporary though it be, would under other circumstances be regarded as morbid; but here we accept it as a part of the physiological process. It is not, however, to be wondered at, that the great modifications of blood supply, of which we have such constant evidence in child-bearing women, should, in those who are strongly predisposed to nervous disease, be productive of actual insanity. How much these changes structurally affect the encephalon may be deduced from the presence of osteophytes or bony plates on the surface of the dura mater and the inner table of the skull; and how much, so to speak, functionally, from congestion and flushings. Nor is it only the quantity, but the quality of the blood which may be a factor. The increase of fibrine and water, and the decrease of albumen, must exercise an influence over the nutrition of cells, and it is possible that the predominance in the individual of one or other of these conditions may determine or condition the nature of the insanity. The most frequent symptoms of the insanity of pregnancy are melancholy and moral perversion: the latter taking the form of dipsomania, the former being accompanied by a suicidal impulse, much less commonly mania of an asthenic type. It is worthy of remark, that psychical disturbance manifests itself most frequently during what are generally considered the critical months of utero-gestation, and that its incidence as to frequency is in inverse ratio to the number of the confinement.

Passing over the mania occasionally occurring during childbed as the head passes the vulva, which is most probably due to temporary congestion consequent on muscular effort, we find a well-marked sequence of mental symptoms following on delivery. This is the true puerperal mania, a name which, when applied to the insanities of pregnancy and lactation, is a direct contradiction in terms. In the large majority of cases of puerperal insanity, the symptoms are those of violent mania—a mania with delirium.

The puerperal maniac has symptoms which, as a rule, cannot be mistaken for any other form of insanity, with perhaps one exception,—mania *a potu*; but even here there are points of diagnosis which are very prominent. The bodily symptoms are at direct variance with the mental. She is pale, cold, often clammy, with a quick, small, irritable pulse, features pinched, generally weak in the extreme, at times almost collapsed-looking. But withal she is blatantly noisy, incoherent in word and gesture; she seems to have hallucinations of vision, staring wildly at imaginary objects, seizes on any word spoken by those near her which suggests for a moment a new volume of words, catches at anything or any one about her, picks at the bed-clothes, curses and swears, will not lie in bed, starts up constantly as if vaguely anxious to wander away, and over all there is a characteristic obscenity and lasciviousness. Suicide is often attempted, but in a manner which shows that it is not the result of any direct cerebration; she may wildly throw herself on the floor, attempt to jump from the window, or draw her cap-strings round her throat, but there is no method about it, it is an impulse, the incentive of which is purely abstract.

This description of course applies to the severest class of cases, but it is taken from the recollection of not a few. Even where the symptoms are not so acute the same tone exists; and the shorter the time of its supervention after delivery, the more acute and marked the mania, and more rapid the recovery.

A careful analysis of a large number of cases of puerperal insanity which had been admitted into the Royal Edinburgh Asylum and the Fife District Asylum showed that, although mania was the most frequent symptom, melancholia was occasionally the indication: but what is of great importance from a pathological point of view, melancholia never appeared within sixteen days after labour, generally manifesting itself in about a month. It further eliminated the fact, that complicated are much more frequently followed by mental symptoms than natural labours; for out of 73 cases of true puerperal mania, 23 (or 30 per cent.) had supervened on abnormal parturition. These two facts taken together lead to the conclusion, that melancholy after labour is not due to the same exciting cause as mania; in a word, that it is not puerperal insanity in the true sense of the word. The various depressing influences of childbed, its various accidents reducing vitality, the sudden return to ordinary physiological conditions, the cessation of normally abnormal physiological conditions, the rapid call for a new focus of nutrition, the translation as it were of the blood to the mammæ, may all be instanced as physical influences liable to act on the brain tissues. The late Sir James Simpson advanced a most seductive theory, that puerperal mania depended on an albuminuric condition, in like manner as puerperal convulsions. As far as my own observations go, and they have been fairly extensive, I have not been able to substantiate this theory; for although I have found albumen in three cases in very slight clouds, still in upwards of 26 no evidence of its presence was obtained. It may be fairly confessed, that the pathology of puerperal insanity is not so definite, or referrible to absolute conditions, as the insanity of pregnancy; but it asserts itself in so strong a manner that we are compelled as physicians to accept it as a special form, although as pathologists we are not able to give such strong reasons for our belief. It is mere hypothesis, still an hypothesis not altogether inadmissible, to suggest that the decadence of those conditions, which had produced such physiological adventitious structures as osteophyte, may be factorial in the induction of insanity. Great difficulties stand in the way of the elucidation of the morbid anatomy of these two forms of derangement. On the other hand, we have in the case of the insanity of pregnancy so many suggestive facts as to actual structural changes, as to lead us to believe that by the study of the causes of the slighter forms of mental disease, we may hope to arrive at a knowledge of those evidenced by more persistent symptoms.

The insanity, when it follows on prolonged lactation, being of a truly anæmic character, falls to be considered under that general condition.

Under evolutional conditions may be fairly comprehended the manifold disturbances of the catamenia—but in a very general manner, inasmuch as they are induced and produced by each and all of the conditions which may be inductive of insanity. It is far too complex a question to enter upon at present, how far the increase, decrease, or absence of the menstrual discharge is influential in the induction of insanity, or how far they are the results of the trophesial influences exercised by the conditions which induce the insanity. This must be relegated to the future, of which I have so often spoken. That the modifications of the discharge are occasional immediate causes of insanity is proved by the fact, that on its regaining its normal amount and quality, the mental symptoms disappear.

A very cursory consideration of the morbid conditions of the system which concurrently affect the brain must suffice at present. But before proceeding to this cursory consideration of individual conditions, I must remark on the absence of abnormal mental manifestations as sequences on certain forms of disease, which, *a priori*, might be supposed to be the most probable immediate causes, but to which in fact they can seldom be referred. I allude in the first place to those chronic diseases which affect the rectum, bladder, uterus, and contiguous parts, such as stone, fistula, cancer of uterus and rectum, stricture with its miserable consequences, and many others which must suggest themselves to you as exercising a most distressing and depressing influence on the nervous system. Of course, we can all remember individual cases in which insanity appeared as a remote consequence on such maladies; but at the first glance, it would be matter of wonder that the instances at our command are so few, were we believers in peripheral irritation as an immediate cause of insanity; and in the second place, I refer to diseases of individual organs, such as the heart, liver, spleen, and kidneys. It might be expected that heart disease would be more frequently inductive of insanity than it is—in fact, it is a question whether it is ever the efficient factor. We are told, to be sure, in certain works on mental disease, that obstructive valvular disease is connected with simple and hypochondriacal melancholy, and that dilatation of the heart is frequent in chronic mania; but these very vague statements carry really no weight with them, as they are unsupported by clinical statistics. Judging from my own experience, I do not think that disease of the heart has any real connexion with insanity as a cause, however much it may have as a result—its early presence in association is a mere coincidence. In like manner, diseases of the liver are, as a whole, innocuous to brain health. We certainly find abnormal livers in insane subjects, but we are almost invariably able to refer the insanity and the liver disease to one common cause. Diseases of the kidneys are decidedly rare amongst the insane, although we have temporary mania or delirium in Bright's disease, probably due to uræmic poisoning.

Much stress has been laid on diseases of the uterus and ovaries, and more especially on tumours of these organs, being the primary factors in the production of insanity. Skae laid down as a special form ovario- or utero-mania; and Dr Wergt of Illnau has described the various morbid conditions of the female organs of generation found on post-mortem examination, and connects with them mental symptoms. Not having met with any case which on mature deliberation could be ascribed to such diseases with absolute certainty, and only with one or two in which a strong presumption was warrantable, I began to doubt that uterine disorders ever exercised a sympathetic influence, or acted on the nervous centres by peripheral irritation to a degree productive of insanity. On consulting some of our leading gynecologists, I found that their observation tended in the same direction; that although cases occasionally but rarely presented indications of melancholy, this symptom was more a secondary than a primary one, and was caused by loss of sleep consequent on depressed emotions, anxiety, and pain. The fallacy of advancing uteromania or ovariomania as a special form arises, I believe, out of the clinical observation that, in certain cases, tumours and other deviations from health in the organs of generation, condition to some extent the nature of the mental symptoms in such a form as climacteric insanity. Women who for years have been carrying tumours, when they arrive at the change of life develop aberration of intellect, and not unfrequently the character of their delusion is marked by sexuality and erotomania; they think they are pregnant, or that they are visited at night by men. It is to this extent only, I believe, that such morbid growths are connected with the insanity; they may condition, but there is no actual clinical proof of their primary influence on causation by peripheral irritation. There is too much of the element of coincidence—of the *post hoc, propter hoc*—involved to allow us to accept utero- or ovario-mania as a pathological entity, for I have known of several chronic cases of insanity with uterine tumours without any sexual symptom. In like manner, we have a few scattered cases of insanity coexistent with disease of the kidneys, bladder, and spleen, but they all want what an art critic would call breadth of colour conjoined with definiteness of outline, which is so characteristic of the great forms of insanity.

From an ætiological point of view it is of great interest, and I believe of great importance, to note that insanity is seldom, if ever, the immediate *result of diseases of individual organs*, or of peripheral irritation, but that it is most intimately associated with those forms of disease which are the manifestations of a general constitutional instability, such as tuberculosis, rheumatism, gout, or syphilis. This fact, if fact it be, points to the implication of the nervous system and of its central organ in the general impairment of nutrition which produces these morbid conditions, and that as each and all of them may manifest itself in various ways and in various regions, insanity and the brain may be the way and the region in which a section of them do manifest themselves. Thus, without overstepping the limit of health, we have the feebleness or the æstheticism of the tubercular, the irritability of the gouty, and the paralysis of energy of the syphilitic. There is, I believe firmly, an hereditary phthisical brain, and an hereditary gouty brain, and an hereditary syphilitic brain; and further, that through such brains the constitutional instability may be evidenced by insanity without any actual outbreak in other parts of the system.

Passing from the general question, we find that in these three great morbid constitutional conditions, insanity is an occasional concomitant, and the psychical symptoms in each case are well marked and defined. We have to thank Dr Clouston for the first description of what he calls the insanity of tuberculosis. His conclusions have very severely been called in question, but I, for one, maintain a belief in their accuracy and value. The symptoms of their form in no way partake of the character of the spes phthisicorum or euphoria, although in some respects there is a resemblance between the want of fixity of purpose of the uninsane tubercular subject and that of the mentally afflicted one. To quote Dr Clouston's own words, the symptoms may be described as "a mixture of sub-acute mania and dementia, with a great disinclination to exert the intellect; occasional unaccountable little attacks of excitement, and a disinclination for work or even amusement; pervading all is a strong tendency to be suspicious." As to the pathological anatomy of this form of insanity, little can be said. Tubercular deposits on the surface of the brain or in the tissues are as rare in the insane as in the sane adult, and as yet the microscope has contributed no fact bearing upon the immediate question, although it presents a hopeful field of research. There are many very interesting points in connexion with tuberculosis associated with insanity which will be fully discussed when we take up the question of the corporeal symptoms of insanity and the influence it exercises in modifying other forms of disease.

In the insanities which are concurrent with rheumatism and gout, we have as perfect pathological entities as are presented in the whole extent of mental disease. If we were now classifying insanity a difficulty would arise as to where these two forms should be placed; whether they should be held to be diathetic or metastatic. This is especially true in regard to rheumatic insanity, for the mental and bodily phenomena are distinctly vicarious one of the other. The metastasis from the general to the nervous system is symptomatized by choreic movements of the limbs, which disappear contemporaneously with the return of inflammatory swellings of the joints or other indications of the disease in the muscular tissues. But if we accept the general principle laid down in the opening remarks on this section, the metastasis must be regarded as a peculiarity of the general condition, and the brain, and more especially its connective tissues, being predisposed like other structures to the morbid influence of rheumatism, is liable to be, in common with them, the seat of translation.

The general characteristics of gouty insanity are very analogous to those of the rheumatic form. The metastasis and vicariousness of the symptoms are the really important pathological indications. Dr Berthier has given us an excellent paper on this subject. He has, however, omitted, I think, one physical symptom of gout, which is an obfuscation of intellect, or rather a semi-paralysis of the thinking powers in those to whom gout clings, without breaking out into acute inflammation. I refer you to Dr Berthier's careful paper, published in the Annales Medico-Psychologiques, in the year 1869. With regard to syphilis, we may accept the changes in the blood-vessels themselves as the cause of insanity amongst its subjects. This interference with the nutrition of the brain appears to act in two ways: 1st, by producing circumscribed tracts of degeneration and softenings which implicate the health of the whole encephalon; and, 2dly, by a general impairment of its nutrition. I lay before you specimens taken from two syphilitic cases who died insane, in which the remarkable changes in the vessels are very well marked. I am not sufficiently versed in the literature of the disease to know whether the appearances you will see have as yet been described, and I shall feel obliged by an expression of opinion on the subject.[1] With the lately published exhaustive lectures of Dr Broadbent on Syphilis before us, it is unnecessary for me to say more on this subject.

It would likewise be a work of supererogation for me to discuss in full the toxic causes of insanity, for all here present are well acquainted with the various theories of the actions of the poison of alcohol, tobacco, and other, so to speak, articles of diet, and of the toxic influence of the various mineral and vegetable poisons on the system. In the instance of alcohol, the poison which is the prime factor of a special form of brain disease, alcoholismus, acute and chronic, a simple physiological experiment at once demonstrates that its application to an exposed nerve results in immediate cessation or paralysis of its energy; and as it is fully proved that it is readily absorbed into the circulation, and thus is applied to the central nervous organ, we have little difficulty in accounting for its physiological and pathological actions. In the same way the results of the abuse of many medicinal agents can be explained up to a certain point. But the whole question of brain-poisoning presents far too wide and too often traversed a field for present consideration. It is only introduced here *pro formâ*; it merely enters its appearance to assert its right to be considered as a cause of insanity. Its pathological histology will be taken into consideration further on in this and the next course.

Turning to anæmia as a cause of insanity, we have a somewhat more complex subject to deal with. The defective nourishment of the brain-cells in the state of anæmia must be accepted as the cause of impairment of their functional activity, for they must suffer concomitantly with other structures, and relatively in proportion to their great vital activity, rendering the regions over which they exercise control all the more liable to the morbid processes which are the known results of the modification of blood constitution. The term anæmia is not used here to imply a condition antithetical to hyperæmia; it does not indicate any mechanical deprivation of blood supply; on the contrary, the amount of blood, such as it is, is not reduced in quantity, possibly it is increased. The temporary mechanical anæmia which results from extreme cold, for instance, produces its effects rapidly—short delirium and profound sleep, recovery from which is accompanied by intense pain. But the anæmia which qualitatively implicates the constitution of the blood is the anæmia which produces more or less permanent results on cerebral health.

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[1] See "A Case of Syphilitic Insanity"—Journal of Mental Science, Oct. 1874, with plate.

In a region of which the recuperative nervous energy is depressed, the phenomena presented by the corpuscular elements of the blood under abnormal qualitative conditions are all the more likely to manifest themselves. Taking a healthy brain, we might expect to see the corpuscles flowing as quietly and orderly as in the perfectly healthy frog's foot or bat's wing, a demonstration which few of us have seen. Taken, on the other hand, a brain whose activity is depressed in a subject whose blood constituents are under the morbid conditions attendant on anæmia, we might expect with almost perfect certainty that the transit of the corpuscles would present the appearance we have all so frequently seen in an unhealthy frog's foot, the aggregation and rolling motion of the red, and the lagging and wandering of the white corpuscles. It is well known that in anæmia, however it is produced, the red corpuscles have a tendency to coalesce, in consequence of the acquirement of an adhesive viscid quality, causing them to hang together, and so preventing the intercurrence of the white corpuscles. When we take all this into consideration, it is not difficult to see how, even more rapidly than in hyperæmia, a condition of stasis can be established in the cerebral vessels.

The psychical symptoms which are manifested in anæmic insanities are, as a rule, characterized by delirious mania—of not long continuance, but intense while it lasts.

Having thus slightly sketched the leading causating influences of insanity, although by no means having exhausted the category (many of minor importance being of necessity omitted), I will conclude this section of my subject by indicating the great chasm which yawns between the pathogenesis and the morbid anatomy of nervous disease, especially of that portion in which abnormal psychical phenomena are the predominant symptoms, *i.e.*, insanity. Clinical observation shows that we have trains of symptoms, more or less defined, associated with certain efficient causes of psychical disturbance; but in the best defined train of symptoms, let me instance general paresis, we have a wide area of divergence. If there is a pathological entity in connexion with what we call insanity, general paresis is that entity; but mark how very widely divergent are its psychical symptoms. True, in nine cases out of ten of this disease, we have a thorough sequence of psychical symptoms, *but there the odd tenth stands* demanding explanation. Take a more common insanity, drunkenness—ordinary ebriety; why is it that one man differs from another in the manifestation of artificial madness? Take, again, the insanity more rapidly induced by anæsthetic agents, why does one patient quietly subside into dementia, whilst another runs rapidly the course of symptoms which we often note in more permanent insanities,—depression or melancholia, violent mania, and terminative dementia? At this point the pathologist must hold his hand; he falls back on the *interrogatio questionis*, idiosyncrasy, or diathesis. But in thus narrowing himself in his imperfect science he is only relatively narrow to his brother worker in the more accurate science of physics. The physicist is compelled to speak of force, but what is idiosyncrasy but a modification of force in the individual? The physicist stands erect when he shows a change in condition of a bar of iron—a comparatively simple body—under the influence of heat, regarding with condescension, with reserve, and with self-complacent criticism, the work of the man who directs his attention to that somewhat more complicated substance, the human frame. It is not probable that the mechanism of psychical action will ever be projected on a chart, or the molecular and chemical changes which accompany it will ever be submitted to demonstration. Helmholtz tells us that there is a limit to the magnifying powers of the microscope—a limit which has already nearly been reached. But another Helmholtz may arise to whom the most abstruse work of his father in science will be but a mere primer.

ARTICLE III.—*On a new form of "Boot" for the Treatment of Talipes Equinus.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

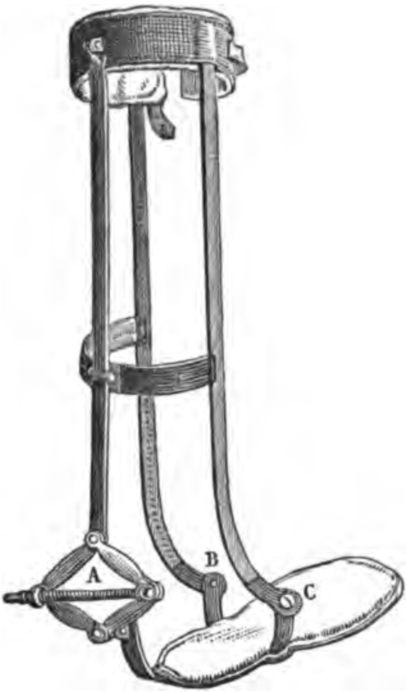
THE boot figured was made, at my suggestion, by Mr Hilliard in September 1870, in connexion with the treatment of a case of talipes equinus. Since then the appliance has proved so successful in treating cases of this deformity in my own practice and also in the practice of others, that I recommend it with much confidence to the consideration of my professional brethren.

I have used it under three conditions:—(1*st*), Without any division of the tendo Achillis. If the distortion is not very severe, the application of the boot without any operation is perfectly successful. One interesting case, treated under this head, was that of a little girl, both of whose feet were affected with the deformity. The apparatus was applied to both feet, and resulted in a complete cure.

(2*d*), With division of the tendo Achillis, and, if necessary, the plantar fascia. In aggravated cases the division of one or both of these structures is usually required, and when the wounds are healed the application of the boot gradually stretches the contracted ligaments and other textures, and so completes the cure.

(3*d*), With removal of the head of the astragalus in addition to the division of the tendo Achillis. In one very aggravated case it was found impossible to relieve the deformity, owing to the displacement and projection of the head of the astragalus on the dorsal surface of the foot. I accordingly excised this portion of bone, divided the tendo Achillis, and, when the wounds were healed, applied the apparatus until the deformity was relieved.

A short description of the boot will be sufficient to make its principle understood. It consists, as shown in the illustration, of a footpiece and leg-support, which are connected together by a movable joint at B and C on each side, so as to allow of flexion and extension of the ankle-joint. The essential part of the appliance is the screw A at the back, which is turned by means of a key. When the screw is turned in one direction it elevates the heel, and the appliance can in this way be adapted accurately to the deformed foot. By turning the screw in the opposite direction, the heel is depressed, and the relief of the distortion gradually and efficiently accomplished by acting upon the contracted tendo Achillis and other textures.



The appliance can either be fitted to an ordinary lacing walking boot, or the foot may be secured to the footpiece by a padded strap crossing over the dorsal surface of the foot and ankle. Sometimes I use a lacing boot of soft leather, similar to that employed in my boot for talipes varus, to secure the foot. In aggravated cases, or in those requiring operation, a combination of the two latter methods is best. The leg is fastened to the leg-support by a broad padded belt passing round the limb at its upper third.

ARTICLE IV.—*Description of a Diprosopus Triophthalmus Monster.* By ANGUS MACDONALD M.D., F.R.S.E., Lecturer on Midwifery and Diseases of Women and Children.

(*Read before the Obstetrical Society of Edinburgh*, 18*th November* 1874.)

THIS very rare and exceedingly curious specimen, of which the accompanying lithograph is a very fair portrait, was brought to me by Mr Crowdy, one of my Dispensary pupils, on the 27th of July last. The confinement had been managed by a midwife, who, however, took fright on seeing the fœtus, and sent for Mr Crowdy's assistance. The medical history of the mother is brief, and really contains nothing which can be construed as bearing on the monstrosity. It runs as follows.

Mrs Munro, æt. 44, was confined on the 27th July 1874 of her eighth child. Her general health has been on the whole good, although she suffered at times from weakness arising apparently from over-lactation. She has been in the habit of nursing all her children for long periods, usually for two years; but on the last occasion for two years and three months. Her children, with the exception of the present fœtus, have all been healthy and well formed, and are indeed all alive and well at the present time, except one who died from measles and whooping-cough at the age of two years.

During the period she carried the present fœtus, she states that she felt herself gradually getting weaker, and at the time of her confinement felt far from strong.

During the latter months of utero-gestation she felt very large and uncomfortable, so that she believed she was about to give birth to twins. She also suffered from severe pain in the right side, which extended down the right thigh, and which made her think she was going to lose the power of her right side.

Her previous confinements have all been normal, and her recoveries speedy and complete. This one was also easy and short, lasting only two hours. The child presented by the feet, and came easily away. The amount of liquor amnii, according to the statement of the nurse and of the patient, appears to have been excessive.

So far as she could calculate, her confinement was a month too early. She distinctly asserts that she felt the motions of the fœtus on the day preceding the labour. After some extra exertion on the day of confinement, the water suddenly came away, and the expulsion of the child followed within about two hours afterwards.

But now to turn our attention to the child, which is the main object of consideration at present.

General appearance.—The fœtus, which seemed to be, as far as the mother could judge, about the eighth month, is fairly nourished and of good colour. Its length is 13 inches, and its weight 3½ lbs. The trunk and extremities viewed anteriorly seem regularly formed

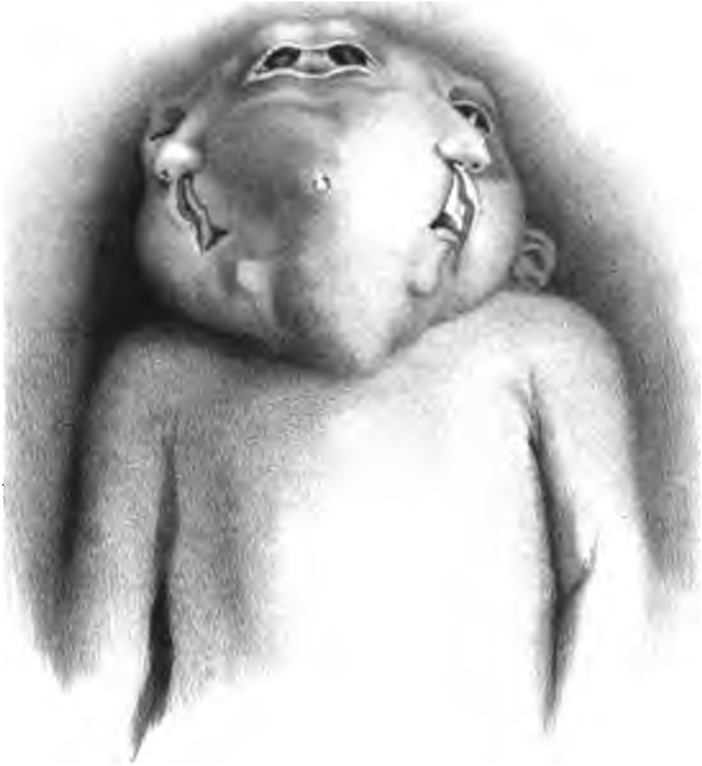


FIG I

Diprosopus Triophthalmus Monster.

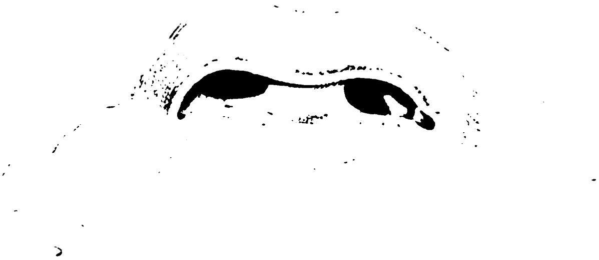


FIG II.

Enlarged Central Eye.

Dr Angus Macdonald.

as well as symmetrical, and proportionately developed. Posteriorly, it is seen that the spinal cavity is very imperfectly closed in, as from the top of the sacrum upwards along the whole extent of the back the skin and other soft structures are entirely undeveloped, in a strip, to the extent of about 2 inches in width, the skin terminating on each side of this area in a free border as if it had been cut with a knife. The arches and spines of the cervical, dorsal, and lumbar vertebræ are also entirely wanting. Immediately within the edge of the true skin on each side run a range of hardened elevations, which look like the transverse processes of the vertebræ. There thus exists a condition of complete spina bifida, the spinal cord being represented by comparatively small nervous cords lying along the floor of the sulcus formed by this open nervous cavity.

The sex of the child was female. The labia minora still protruded in front of the labia majora. The anus and vagina were perforate, but the nails on the fingers and toes were still short of the tips of the digitals.

The neck both before and behind seemed very imperfectly developed, and remarkably shortened in the direction of its longitudinal axis.

Head.—On the posterior, superior, and lateral aspects, the bones of the skull are entirely absent. The posterior and middle lobes of the cerebrum, as well as the anterior, seem wonderfully complete. The cerebellum, however, seems to have been originally wanting, or else to have been destroyed during the labour.

Face.—The facial aspect, however, of the head, presents the most distinctive and most obvious peculiarities in this monstrosity.

It is essentially double throughout, but the inner aspects of the two faces are closely united throughout along the mesial line. The integration has proceeded so far as to form, instead of two eyes placed one on each side of the mesial line, one large compound eye with two eyeballs closely united.

On examining this double face we find that the apposition of parts is not according to parallelism, but such that, if we produce downwards in a transverse plane two straight lines representing the directions of the fronto-mental diameters of the faces, they meet at a point either in or very near the umbilicus of the fœtus.

The double face is separated from the trunk anteriorly by no distinct neck, but only by an irregularly transverse notch.

The lower maxillæ are to be felt separate from one another, and both apparently perfect and well ossified; on each side a slight elevation of surface indicates and represents the chins.

Along the mesial line the union is complete, and the connecting surface even, except at one point midway between the chin and the central eye, where there is a slight depression.

The mouths viewed anteriorly form two irregularly triangular slits, the bases of which are inferior and the apices turned towards the inner ala of each nose. This appearance is effected by the presence of a large hare-lip in both mouths; the lower lip and lower maxilla forming the base of the triangle being normal, whereas the upper lip is very deficient and gapes widely, and a cleft passes backwards and inwards through it as well as through the entire mass of each of the molar and palatal arches at the inner angles of the two mouths, so as to produce both hare-lip and cleft palate, and thus establish free communication between the nasal and oral cavities along their whole length, in connexion with each face.

On passing two probes, one along each oral cavity, it is found that they meet posteriorly in a common pharyngeal cavity.

On the outer aspects of the faces on each side is a well-formed and apparently perfect eye. An ear is also present on each side, and seems regularly formed.

A small portion of both the frontal bones is present about ¾ of an inch in depth and about 1½ inches in breadth, and these are taken up in the formation of the orbital processes. No part of a frontal suture is to be felt in either portion of frontal bone.

The most remarkable feature in the double face, however, is the large central double eye.

This eye is composed of two large eyeballs, each with a separate iris and cornea, but united internally by a common sclerotica, or, if not common, at least closely adherent sclerotic coats.

Each cornea is about 4 lines in diameter, and their inner edges are separated by about 2 lines of sclerotica. The conjunctival covering is single, and there is a common upper and under eyelid for the compound eye. In its formation, however, there is seen, in its wavy outline, a visible tendency to differentiation into two eyelids.

The trunk and extremities presented nothing abnormal externally, with the exception of the spina bifida and defective neck. But the visceral development, for a careful dissection of which I am indebted to Mr Thomas R. Ronaldson, is very peculiar and highly interesting. It is as follows:—

"A free crucial incision was made into the body anteriorly, the vertical part running from the upper end of the sternum superiorly to the symphysis pubis inferiorly, the transverse part running from side to side on a level with the eighth rib.

"On separating the four flaps thus made, two upwards and outwards, two downwards and outwards, the *diaphragm* was found in an imperfect condition. It was attached normally on the right side, but a little beyond the middle line it terminated in a round free muscular border which lay in a furrow on the upper and anterior surface of the enlarged left lobe of the liver.

"This border, on being traced more deeply, extended backwards and to the left so as to be attached to the last rib on the left side and to the rudimentary spinal column, lying over the aorta and ing a loop through which the œsophagus passed.

"To its upper surface to the right of the middle line was attached the pericardium, the left side of the pericardium joining the diaphragm towards the mesial plane.

"To its under surface the liver was attached by the coronary ligament, which was normal on the right side but deficient on the left.

"From this condition of the diaphragm it followed that there was a communication on the left side between the thoracic and abdominal cavities.

"Parts lying superficially, and seen on opening the body:—

"1. *Thorax.*—In the right side of the thorax the heart, contained within its pericardium, was exposed.

"Superiorly, and lying on it, the thymus gland was situated, principally in the right side, but projecting also into the left side of the thorax. The enlarged left lobe of the liver occupied the greater part of the left side (*superficially*), hiding the other organs from view; but superiorly and externally could be seen a few coils of small intestine. The lungs were out of sight.

"2. *Abdomen.*—The liver occupied the right hypochondriac, right lumbar, right iliac, umbilical, epigastric, and part of left hypochondriac regions, while a large part of its left lobe was contained in the left side of the thorax, its anterior border running almost vertically downwards from near the apex of the left side of the thoracic cavity to the umbilical region, and then more obliquely towards the right iliac region.

"From its under surface the flaccid gall-bladder projected in the umbilical region. To its anterior surface, a few lines from the edge, the umbilical vein entered its surface (in the epigastric region).

"The left hypochondriac, left lumbar, left iliac, and hypogastric regions were occupied by coils of large intestine (corresponding to transverse colon, descending colon, and sigmoid flexure) loaded with meconium.

"Inferiorly to the liver, and in the hypogastric region, the bladder and uterus in addition projected into the abdomen.

"Parts lying deeply, and seen on turning aside the superficial parts:—

"1. *Thorax.*—On separating and turning aside the superficial parts in the thorax, the lungs were found firm and collapsed, lying on each side of and touching the pericardium.

"On turning up the left lobe of the liver, the stomach was found immediately beneath it.

"The œsophagus, after traversing the posterior mediastinum, passed through a loop of the diaphragm as previously referred to. It then turned to the left almost directly to enter the cardiac end of the stomach, which lay curled up so as to form the greater part of a circle, the cardiac end lying above the level of the diaphragm and touching the left side of the pericardium as high as the third or fourth costal cartilage.

"The pylorus was situated about three-eighths of an inch below the entrance of the œsophagus and in the abdomen.

"The concavity of the small curvature lay directly to the right, and in it lay a lobule of liver substance.

"To the cardiac end of the stomach and superiorly to it, the spleen, about the size of an almond, was attached (by gastro-splenic omentum), and lay in contact with the left lung, between it and the stomach.

"On raising the stomach and spleen, the part of the large intestine corresponding to the transverse colon was found, while lower down it appeared from behind the stomach and lay on a level with it and covered by the great omentum. Also behind the stomach as well as the large intestine, but projecting also superiorly and externally, the great mass of the small intestine, closely coiled up and apparently empty, occupied the greater part of the left thoracic cavity.

"2. *Abdomen.*—On turning aside the liver, which occupied the greater part of the abdominal cavity, the duodenum was seen lying behind it, in the epigastric and left hypochondriac regions. To the left of the duodenum lay three vertical tiers of large intestine. Anteriorly, and seen on opening the abdomen, was a part corresponding to the transverse colon; behind lay the caput cæcum coli, and still further posteriorly part of the descending colon was situated. These parts, as well as a few loops of small intestine, lay in the left hypochondriac region. The left kidney, with its suprarenal capsule, lay in the left lumbar region behind the descending colon; the right kidney, with its suprarenal capsule, lay in the right lumbar region behind the liver.

"*Details as to the Arrangement of the Intestine.*—The duodenum turned towards the left after leaving the pylorus, and, after receiving the ductus communis choledochus, passed beneath the part of the large intestine corresponding to the transverse colon and the lower part of the ileum, and joined the jejunum. The first part of the jejunum ran upwards, and, along with the ileum, formed the coiled-up mass of small intestine which lay above the level of the diaphragm, and which occupied the greater part of the left thoracic cavity. The ileum entered the caput cæcum coli on a level with the diaphragm. The caput cæcum, with the part corresponding to the ascending colon, passed downwards and to the right as far as the epigastric region, then turned upwards on itself and became continuous with the transverse colon, which passed directly upwards covered by great omentum till it lay behind the stomach in the thorax; turning to the left, it passed beneath the neck of the coiled-up mass of small intestine. It then turned downwards and passed immediately over the bundle of muscular fibres representing the left side of the diaphragm, becoming now the descending colon, and, occupying the left hypochondriac, left lumbar, and left iliac regions, the latter as the sigmoid flexure, it entered the pelvis as the rectum.

"Genito-urinary organs natural."

This kind of monstrosity is exceedingly rare. Speaking of diprosopus or double face in general, August Förster, who is the latest, and perhaps best, authority on monstrosities, states that out of 500 cases of double monstrosity noted by him, only 29 were diprosopi.

But the term diprosopi includes five sub-varieties, which may roughly be distinguished in the following manner:—

1. Diprosopus diophthalmus, when only two eyes are differentiated.
2. Diprosopus triophthalmus, when three eyes are differentiated.
3. Diprosopus tetrophthalmus, „ four „ „ „
4. Diprosopus triotus, „ three ears „ „
5. Diprosopus tetrotus, „ all four „ „ „

So that when we subtract those cases recorded under the other four sub-varieties of this monstrosity, we come to reduce its frequency very much indeed.

The cases collected to by Förster are six in number, gathered from a wide range of literature.

A curious fact regarding these monstrosities is noted by Förster, and this is, that the female sex very largely predominates. Of the twenty-nine cases to which he makes reference, six were males, sixteen females, and in the case of seven the sex was neglected to be noted. An additional point of some little interest might also be referred to here, viz., that in this case there was undoubtedly a degree of hydramnios, a pathological condition which is known to occur much more frequently in connexion with female than with male births.

The diprosopi are not capable of living, as they have never been known without such an amount of defective development of the brain and spinal cord as to exclude the possibility of prolonged existence. It is also to be noted, that though the rest of the body may appear to be well developed—and so much is this the case that Förster says, "As a rule it is well built"—yet a careful dissection of the visceral arrangements might be expected, as in the present case, to reveal conditions incompatible with vitality, although a superficial external examination might not lead one to expect such derangements.

As to the origin of such a monstrosity, it seems to me, in the present state of our knowledge, idle to speculate.

As far as the mother was concerned, there seems no cause, except that her health was under par. But if the embryo of every woman whose health during pregnancy was poor, took to splitting up in this odd fashion, how frequent, instead of how rare, would double monstrosities be!

We are, however, bound to record such cases, in the hope that they may furnish data for some future worker, to aid him in elucidating those obscure causes interfering with the early differentiation of the embryo, which terminate in producing monstrosities. I meantime have only to say that there is little, if any, scientific ground for the common belief that monstrosities frequently arise from mental impressions received by the mother. In this particular case, there is no fact to support such an idea. I am sorry that the desire to possess this singular specimen as an inmate of my museum, in as complete a condition as possible, has as yet proved too strong to allow me to get the face dissected, much as I would wish to do so, on many grounds.

ARTICLE V.—*On Prickle Cells in the Capsule of a Fibrous Polypus of the Uterus.* By Dr J. R. HARDIE.

(*Read before the Obstetrical Society of Edinburgh, 18th Nov.* 1874.)

THE patient from whom the polypus about to be described was obtained was a woman 53 years of age, with a pasty-looking, blanched countenance, old-looking for her years, who was admitted to Ward XVI., Royal Infirmary, on the 18th of August, complaining of occasional severe floodings and a constant copious white discharge. The floodings commenced about six years before admission—that is to say, when the woman was 47 years of age; the bloody discharges lasted generally about a month, and weakened her very much; the interval between them varied from two to four months; she had her last flooding in June. She has had five children, the last sixteen years ago.

On examination per vaginam, a large rounded firm tumour; about the size of an apple, was found occupying the vagina; it could be traced into the cervix uteri, which was dilated round it; it appeared to be attached to, and to spring from, the cavity of the cervix.

The growth was removed by means of vulsella and scissors by Dr Matthews Duncan on the 20th. The patient, with rapidity, regained health and strength, and was dismissed quite well on the 1st of September. It is interesting to note the unusual age at which the flooding commenced, 47—an age at which we are usually warranted in encouraging a patient suffering from bleeding from a fibrous tumour of the uterus to expect a cessation, or at least a diminution, of the floodings; the intervals likewise would seem to indicate that they had no connexion with ovulation.

On dividing the tumour (to ascertain roughly its nature), it presented the usual appearances of a true fibrous polypus. Its investing capsule was thick. At, and in the immediate neighbourhood of, the presenting point, the capsule was observed to present a rough irregular papillary appearance, which, on microscopical examination, was seen to be caused by an irregular denudation of the superficial layers of the flattened epithelium of which the greater thickness of the capsule consisted.

On microscopical examination, the polypus was seen to consist of fibrous and muscular elements—the former predominating.

The capsule was composed of numerous layers of epthielium, flattened on the surface of the growth, and becoming more globular in the deeper layers. These epithelial cells have hair-like processes proceeding from them, which are more distinct on those on the surface, and disappear gradually as you proceed inwards; they are seen to interdigitate, but not in the pronounced manner in which processes of a similar nature, which exist in the epithelium of the skin or in some forms of cancroid, are observed to do. There is evidently a close connexion between these cells and the prickle or rib cell first described by Schultze as existing in the skin.

ARTICLE VI.—*Clinical Lectures on Diseases of the Heart.* By GEORGE W. BALFOUR, M.D., F.R.C.P.Ed., Physician to the Royal Infirmary.

On Incompetence of the Aortic Valves, with Special Reference to the Progress of the Disease, its Prognosis, and Treatment.

GENTLEMEN,—In my former lecture on Aortic Incompetence[1] I directed your attention specially to the diagnostic significance of the murmurs accompanying it, because it is from them we learn the comparative freedom of the regurgitation present, upon which, in certain conditions, much of the prognosis and many of the consecutive phenomena greatly depend.

When regurgitation takes place through the aortic valves, the left ventricle, during its diastole, fills from two sources instead of one—from the aorta as well as from the auricle. The first and most immediate effect of this is a sudden diminution of the arterial tension, which impresses its character on the pulse in exact proportion to the amount of regurgitation present—a fact which shall be presently more particularly referred to. The second effect of this diastolic filling of the ventricle from two sources is, that when the systole of the auricle takes place, there is produced, first, a greater or less over-distention of the left ventricle; and, second, an incomplete emptying of the left auricle. The result of the latter is a greater or less amount of passive congestion of the lungs, revealed—as I have already pointed out, every obstruction to the onward flow of the blood through the heart, that is, every passive congestion of the lungs from a cardiac cause, is revealed—by an accentuated pulmonary second sound.[2] Once started, this passive congestion of the lungs must, of course—if no compensating change were possible—go on increasing with each cardiac pulsation, until at length it affected the right ventricle, over-distended it, opened up the tricuspid valve, and ended by producing passive congestion of the general systemic veins, terminating in general dropsy, and a gradual transference of the blood-pressure from the arteries to the veins, till the arterial blood-pressure became reduced below that compatible with life, and death ensued under symptoms of a gradually increasing asthenia.[1] This is, in fact, a very common mode of death in other forms of cardiac disease; but it is a very rare one in aortic incompetence, because in it the lesion which obstructs the onward flow is of such a character, as already mentioned, as to produce, directly and at once, such a fall in the arterial blood-pressure as is only brought about by other lesions indirectly, and after a long lapse of time.

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[1] Ed. Med. Jour., Sept. 1874, p. 193.

[2] *Op. citat.*, June 1874, p. 1077.

In all cardiac lesions, however, there is a tendency to postpone this ultimate result for a longer or shorter time by the production of secondary changes in the heart itself, which more or less perfectly compensate these lesions; and it is the object of treatment so to further or to regulate these changes as to procure as perfect a compensation as is practicable, and to maintain its efficiency as long as possible.

In aortic incompetence, the overloaded condition of the heart gives rise to various distressing sensations, revealed by uneasiness or pain in the cardiac region, with irritability of the organ, evinced by palpitation on the slightest provocation, direct or reflex, as the most trifling exertion, or the mere ingestion of food; and the first step in the relief of these phenomena is the accommodation of the left ventricle to the increased supply of blood it is required to contain. This is usually readily brought about by the elasticity of the muscular structure of which it is composed. The first step towards compensation is therefore dilatation of the left ventricle. Inasmuch, however, as simple dilatation of the ventricle can only provide a receptacle for the blood delayed, and not a means of restoring it to the circulation, this first step would postpone the inevitable end only for a very short period, and must itself be compensated before the balance of the circulation can be so efficiently restored as to render the lesion mute. To this end the dilated ventricle hypertrophies; *pari passu* with the dilatation of its cavity, its walls become thicker and more powerful. The mechanism by which this is brought about may not be very clear, but the fact is patent to all; and the result is, that as the left ventricle dilates it also hypertrophies, and that in cases of disease in which regurgitation is only comparatively slowly developed, this all takes place with so little disturbance of the cardiac or of the general circulation, that it is often only after the lapse of many years—frequently

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[1] "Systemic death consists in decline of aortic pressure. This decline may occur rapidly, as in syncope; but usually, even in deaths by violence, it is very gradual. In deaths from disease it may last (gradually decreasing) for days, weeks, or even months."—*Handbook for the Physiological Laboratory*, p. 209.

passed in strenuous labour—that the patient learns to his astonishment that he has in all probability been for so long the subject of heart complaint.[1] On the other hand, when regurgitation has been suddenly developed in an extreme degree, as in cases of accidental rupture of one or more of the segments of the aortic valves, the immediate effect has been great disturbance of the cardiac circulation, and complete compensation has never been perfectly developed, the patient continuing to suffer till in no long time death has ensued.[2]

When aortic incompetence has been developed as the result of local disease in a moderately healthy subject, in whom dilatation and hypertrophy of the left ventricle have gone on, *pari passu*, in such a way as to develop and maintain what is called a perfect compensation for the existing lesion, a time at length arrives when this is no longer possible. I may be unconsciously doing injustice to those who have already written on this subject, but it seems to me that they have assumed that when this so-called perfect compensation is once attained,—it may be after a longer or shorter period of cardiac perturbation,—it is maintained, accidents excepted, till nutritional changes in the heart walls render its longer continuance impossible.[3] If this view, however, be correct, too much attention has been bestowed upon what we may call the vital forces, and too little upon the simple mechanical agencies involved in aortic regurgitation—agencies which are, however, of vast importance in producing injury, and which must be duly attended to and counteracted in our therapeutical attempts to

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[1] I found this statement on the fact that I have seen many cases of aortic incompetence in whom the compensation had been accidentally or unavoidably ruptured twenty years or more subsequent to one single attack of rheumatic fever, from which in all probability the lesion dated, coupled with my own knowledge that some have suffered for quite half that time from aortic regurgitation without being aware of it.

[2] Balthazar Foster, in his *Clinical Medicine*, p. 139, says, that four years and a half covers the date of death in all such cases of traumatic lesion of the aortic valves. There are many clinical facts to prove that it is not the freedom of the regurgitation, as he supposes, but the suddenness of its occurrence, which favours this untoward result.

[3] Thus Jaccoud says—"Quelque parfaite que soit la compensation des lésions cardiaques, elle est temporaire; l'équilibre artificiel au moyen duquel elle maintient une circulation à peu près normale peut durer des années, mais il faut qu'il se rompe, cela est fatal; le tissu musculaire du cœur atteint dans sa nutrition subit à la longue une transformation régressive, généralement graisseuse, et lorsque cette altération secondaire, dont Paget et Stokes ont montré toute l'importance, présente une certaine étendue, le ventricule, quoique augmenté de volume, quoique hypertrophié en apparence, ne contient plus assez d'éléments contractiles pour faire face au travail excessif que la lésion lui impose; sa force propulsive diminue, il se vide mal, l'évacuation du système veineux est par suite gênée, la compensation est détruite, et comme cette rupture résulte d'une condition anatomique sur laquelle nous n'avons aucune prise, il faut que le cœur s'arrête, et que le malade succombe."—*Leçons de Clinique Médicale*, Paris, 1869, p. 215.

alleviate the mischief produced by them. Thus, when a leakage, however trifling, has once been established through the segments of the aortic valve, the effect of this—with the patient in the erect position—is, according to Pascal's law,[1] that the interior of the left ventricle is being constantly dilated by a force equivalent to the weight of a column of blood the height of the cranium above the heart, and of the diameter of the ventricular lumen. From the moment that leakage is once established till the cessation of life, this hydrostatic pressure never ceases, though it may be modified by position; its action is not only continuous, but it increases, in accordance with the law referred to, *pari passu* with the gradual dilatation of the ventricle;[2] while all that the organism has to oppose to this powerful dilating force is the practical exercise of that well-known but inexplicable law, whereby all hollow involuntary muscles hypertrophy and increase in strength in proportion to the amount of any obstacle preventing the exercise of their function (Paget). A due consideration of what has just been said will sufficiently explain the danger to which such patients are subjected, as well as the manner in which nutritional derangements, either general or local, may increase that danger. For instance, if, at the moment that aortic leakage is established, the system is in a depressed condition of nutrition, there may be a difficulty, or even an impossibility, of establishing compensatory hypertrophy, and the result may either be a more or less prolonged period of cardiac disturbance, or sudden death from asystole. And similar results may arise from a similar cause at any moment—even long after compensation has been established—during the persistence of the leakage; that is, at any after-period of the patient's existence. It is, however, in the history of this lesion subsequent to the establishment of compensatory hypertrophy that a great part of its interest lies, especially in regard to the influence of local nutrition. If we regard the compensation, once established, as perfect and complete, then it is obvious that, apart from accidental disturbances, we must wait the development of local nutritional changes before we can have any disturbance of the compensation. If, however, we look upon the compensation as always incomplete, though practically sufficient for the carrying on of the circulation, then a time must at length arrive when the compensation is ruptured from purely physical causes, and that quite independent of either general or local disturbances of nutrition; and it seems to me that this is the most usual case.

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[1] Pressure exerted anywhere upon a mass of liquid is transmitted undiminished in all directions, and acts with the same force on all equal surfaces, and in a direction at right angles to their surfaces.—Vide *Traités de l'Equilibre des Liqueurs*, etc., par Blaise Pascal, Paris, 1763.

[2] In striking contrast to this view, it is somewhat singular that Rindfleisch says of this same lesion—"This dilatation is a result, not of the insufficiency, but of the stenosis."—Vide *Manual of Pathological Histology*, New Sydenham Society's translation, vol. i. p. 291.

In aortic regurgitation there are three possible conditions of the left ventricle, for it is to the state of that cavity that our inquiry in this matter is practically limited.

1st. We may have a condition of over-compensation, in which the left ventricle is hypertrophied in excess of its requirements, and which is said to give rise to many disagreeable and possibly dangerous symptoms. This is a state of matters which is constantly referred to by all recent writers on the subject. It must, however, be one of extreme rarity, if it occur at all. In my own experience I can only recall one case of aortic regurgitation in which I ever had even a suspicion of such a state of matters, and that occurred so long ago that I am by no means certain that, were a precisely similar case to come before me now, I would acknowledge it as a definite example of over-compensation. At all events, I know not where to find recorded any well-authenticated case of the kind; and I am persuaded that for all practical purposes we can well afford to discard all consideration of such a condition.

2d. The balance of compensation may be perfect; the amount of hypertrophy may be just sufficient to counterbalance the dilating effect of the regurgitation, and with a slightly dilated and hypertrophied heart the circulation may go on as before, till, from accidental or inevitable nutritional changes, the compensation is ruptured, and disturbance of the circulation sets in.

3d. We may have, from the moment that aortic leakage sets in, a dilating power acting on the left ventricle, which, from physical causes, must gradually increase. This dilating force acts as a hindrance to the onward current of the blood; the muscularity of the left ventricle reacts against this obstacle, and for a time successfully, so far as the maintaining of the circulation is concerned, but always at a disadvantage. The dilatation which commenced the organic disturbance continues ever in advance, the compensating hypertrophy which followed continues ever to lag behind, till, apart from any nutritional disturbance whatever, a point is reached when asystole is threatened, because the weight of the dilating fluid is in excess of the contractile force of the ventricle. At this moment it is fortunate for the patient that the increased weight of blood is, from physical causes, always accompanied by increase of dilating power, so that just at the moment that ventricular paralysis is threatened, a way of escape is prepared, the segments of the mitral valve give way, and, by regurgitation into the left auricle, the overburdened ventricle is relieved and the inevitable end postponed. Every one must recognise in this latter sequence of events the ordinary history of a case of aortic regurgitation, and must also be fully aware, that long after the occurrence referred to, a patient may die with his ventricular muscle in a state of comparative health. We must all, therefore, be fully prepared to acknowledge the paramount importance of Pascal's law in the sequential production of the organic changes consequent on aortic regurgitation, quite apart from nutritional changes.

But though the hydrostatic influence developed by the imperfect condition of the aortic valve inevitably gives rise to the conditions referred to, these may be precipitated by any general or local disturbance of nutrition. Whenever from general debility the hypertrophy of the left ventricle is interfered with, asystole and sudden death may occur at a very early period of the affection, or over-dilatation and mitral regurgitation may take place at a similarly unusually early period. This may be afterwards compensated, but is probably always followed by an exceptionally early fatal termination. Similar results are threatened at any period of the disease when general nutrition is accidentally interfered with by privation, overwork, or any febrile condition, so slight even as an ordinary catarrh. It is under these circumstances that the disease, if it have ever been mute, ceases any longer to be so; cardiac disturbance sets in, and the patient comes under treatment. In these cases the ruptured compensation may still be restored by appropriate management, and the patient may be reinstated in comparative health. It is otherwise, however, when the nutritional disturbance is local. Judicious treatment may still prolong life, but efficient compensation can no longer be restored, and though the end may be postponed, it is inevitable. To understand why this must be so, we must glance cursorily at the mode in which the heart is nourished, and consider the influence of aortic regurgitation upon it. The importance of the heart as the central organ of circulation, and the amount of work which, as such, it is needful for it to perform, coupled with our knowledge of the paramount importance of food in relation to work with respect to organic frames and their constituent organs, enable us readily to understand how any defect in the nutritional qualities of the blood, however brought about—by old age, cachexia, or disease—may impair the working power of the heart; and when we know that this impaired contractile force is opposed to an obstacle which from physical laws must, if uncompensated, still further endanger the physical means by which that force is applied, we can easily understand the evils to which a patient in this condition may be exposed. But all the ills of spanæmia, or a general defect in the nutritional qualities of the blood, may be emulated or surpassed by any positive interference with the local supply of blood even of good quality, and these are of course still more readily produced when, as in this affection, both these causes are at work.

The heart receives its supply of arterial blood from the coronary arteries, during the systole of the left ventricle, however, two causes conspire to prevent arterial repletion: *1st*, The blood-current flows at right angles to the orifices of these arteries, so that only a small portion of the systolic wave can be supposed to enter them; *2d*, The ventricular tissue is at this time so firmly contracted as still further to limit this systolic wave supply. And to these hindrances we have to add the anatomical fact, that in a considerable proportion of individuals the coronary arteries open so far within the valvular zone, that at the moment of ventricular systole their orifices are closed by the segments of the aortic valve thrown back on the wall of the artery, so that any possible systolic supply is completely shut off, and in them the whole, as in all cases the most abundant, supply of nutritional blood to the heart, must take place at the moment of ventricular diastole, when the closed semilunar valves direct the aortic systolic blood-current into the cardiac tissue already prepared to receive it by its diastolic relaxation. But when the semilunar valves have given way, and ventricular regurgitation has been established, the aortic systolic wave entering the coronary arteries is not only diminished by the amount of regurgitation present, but has its tension correspondingly lowered, the result being an imperfect flushing of the heart's tissue with arterial blood at a pressure below the normal, so that we have not only a defective supply of the nutritive material, but an imperfect application of it, from the persistent maintenance of an increased venous tension, of local origin, which produces a stasis of venous blood in the radicles of the veins and in the capillaries, and thus shackles the cardiac nutrition in its final act as effectually as the regurgitation does in its initial one, the mere admission of the nutrient blood. To this we must add the additional hindrance opposed to the entrance of arterial blood into the cardiac muscle by the dilating force applied to its interior. The result of all these causes combined is, that the cardiac muscular tissue is in a state of persistent venous congestion with defective arterial supply, both being in exact proportion to the freedom of the regurgitation, and, as already seen, specially so when the mitral segment of the aortic valve is traumatically affected, so that the branch of the coronary artery lying behind it is almost wholly deprived of its blood-current. Of course, any spanæmic condition of the blood generally, or any atheromatous obstacle at the mouth of one or other of the coronary arteries, renders these causes doubly effective. Under these conditions brown atrophy of the cardiac muscle may occur; but the most frequent alteration of all is yellow atrophy (fatty degeneration), affecting chiefly that part of the ventricle where its nutrition is most effectively shackled by the pressure of the dilating force, viz., the sub-endocardial layer of the muscular substance both of the ventricular wall and of the musculi papillares.[1] Along with these changes there is usually found in such hearts an unusual predominance of fibrous tissue. This by some is regarded as also the result of muscular degeneration; but inasmuch as it is found in other organs, the liver, spleen, and kidney, where no muscular tissue is present, but which have been subjected to similar venous congestion, it is possible that the true pathological cause of this form of cardiac fibrosis has yet to be discovered.

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[1] Rindfleisch, *op. cit.*, p. 274.

We can readily understand the important influence which these changes in the ventricular muscle exercise upon the prognosis of the disease; and as these changes depend upon imperfect nutrition, and that again upon the amount of regurgitation present, we see how important a matter it is to recognise this accurately. The means of doing this I have already pointed out.[1]

The pulse of simple aortic regurgitation is, as was first described by Sir Dominic Corrigan, something entirely *sui generis*. In its most typical form it is large and full, quickly projected against the finger, and falling off rapidly. Sometimes it communicates a vibratile jar to the fingers. Scarcely has the shock of the impulse been perceived than the arterial tension sinks to a minimum, from the aortic reflux, and the sensation vanishes. This pulse—*pulsus celer et infrequens*—has been likened to the successive propulsion of shot against the finger, or to the jarring shock of the toy called a water-hammer. It is always regular, and its characteristics are increased by raising the extremity (arm or leg) in which we feel it perpendicular to the body as it lies horizontal, or by raising the arm above the head in the erect posture. But these characteristic peculiarities of this form of pulse vary with each case, and may be greatly modified. Thus great obstruction, which also diminishes regurgitation, may lessen the distinctness of the jar very considerably, while a certain amount of anæmia often increases it; and consecutive or coexisting mitral disease frequently impresses upon it the mitral characters of smallness, feebleness, and irregularity so strongly, that its own peculiarities are entirely lost. Occasionally in these circumstances elevation of the arm brings out with tolerable distinctness the collapse of aortic regurgitation; but this is not always easily detected, and cannot be relied upon unless well marked. When it is distinctly recognisable we are, however, justified by the twofold nature of the pulse character in assuming the existence of a double lesion—aortic and mitral.

If we feel the apex beat of the heart and either of the radial pulses simultaneously in a perfectly healthy individual, it will be found that they do not beat simultaneously, but that the radial pulsation lags behind that of the heart by a distinctly appreciable interval of time, which may vary slightly in each individual, with special relation to the rapidity of the heart's action; but the pulse in health always precedes the second sound. This delay of the pulse is due to the fact that the arteries are elastic and not rigid, and that the blood moves along them as a wave, and not merely as a mass, one end of which is extruded as the other intrudes.[2] So long as the arteries are fairly distended, trifling variations exert no marked influence on the state of the arteries, or on the rapidity of the blood-wave; and the same is the case in regard to the force of the heart's contraction. When, however, the heart's impulse is greatly increased, and especially if the blood-wave is enlarged, then the elastic arteries both lengthen and dilate. As in aortic regurgitation, the left ventricle is both hypertrophied and dilated; the impulse is greater and the blood-wave larger than in health. The arteries are therefore longer (more tortuous) and more dilated than in health, so that the pulse is more visible, and attended by more arterial movement than usual (locomotive pulse). But as these phenomena may be due, though usually in a less degree, to other causes than aortic regurgitation, they are not distinctive of it, though their presence certainly leads us to suspect its possible existence. In aortic regurgitation, however, the arteries are not only longer and more dilated than usual, but, from the reflux into the ventricle, they are also less distended; the blood-wave is therefore longer of reaching its maximum, and the pulse is delayed.[1] This delay of the pulse in aortic reflux is invariable, as was first pointed out by the late Dr Henderson nearly forty years ago,[2] the only apparent exceptions being those cases in which the delay is so great that the radial pulse coincides with the ventricular systole immediately succeeding that which has produced it; but any possible mistake in such a case may be corrected by tracing the pulse-wave from the ventricle through the carotid and brachial arteries to the radial artery, which may be fairly enough done by the hand, but may be quite accurately determined by the electric methods devised by Dr M'Kendrick[3] and by Dr Landois.[4]

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[1] *Vide* Ed. Med. Jour., Sept. 1874, p. 193.

[2] It is extremely interesting to find Erasistratus as accurate in this matter as he is in his pathology, and Galen as wrong as he is—as usual—dogmatic. e all the more safe in making these statements that it is from Galen's

Whenever, therefore, we have a visible, locomotive pulse, which is delayed beyond the commencement of the relaxation of that ventricular systole which has originated it—as marked by the occurrence of the pulmonary second sound—then we have to do with an aortic regurgitation which is probably great in proportion to the

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writings alone that we learn his great opponent's views: "Nam quod ait Erasistratus, semper priorem (scil. arteriæ partem) moveri prius videri, sensus non probat."—Galen de Puls. Different., lib. i. c. 25. And again: "Quod Erasistratus mentitur de rebus evidentibus. Omnes enim clare cernunt, omnes partes arteriarum *eodem distendi tempore*."—Galen de Causis Puls., lib. ii. c. 8.

[1] The pulse-wave seems to pass through the arteries like a tidal bore wave, as was first pointed out by Mr Colt.—London Med. Gazette, vol. xxxvi. p. 456. Practical experiments in proof of this are as yet wanting; but the modern idea (*vide* Handbook for the Physiological Laboratory, p. 228), that the arteries dilate and contract again in segments, to pass on the blood-wave, is open to the objection that the primary contraction of the aortic segment would necessarily close the aortic valves long before the pulse-wave had reached the distal arteries, which we know is not the case.

[2] Ed. Med. and Surgical Journal, vol. xlviii., 1837, p. 369.

[3] Ed. Med. Jour., July 1874, p. 8.

[4] Die Lehre vom arterien puls, von Dr Leonard Landois, Berlin, 1872 p. 302, etc.

delay of the pulse. This form of pulse is represented graphically as in the two following figures:—

FIG. 1.

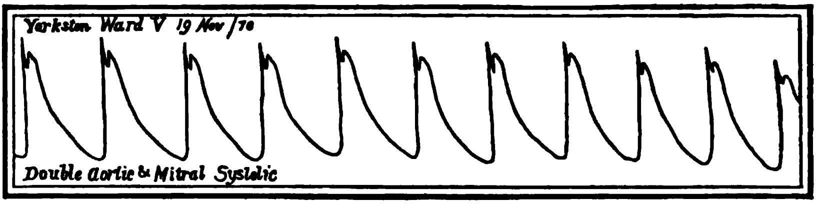
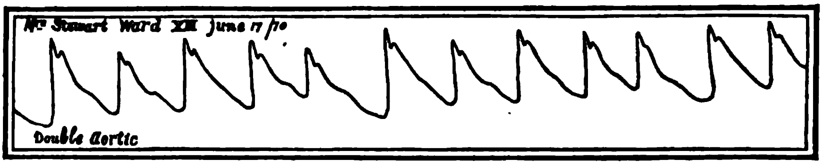


FIG. 2.



In these tracings there is nothing to indicate undue postponement of the pulse. I have just pointed out how that is to be ascertained —here we have only noted the local succession of events as they are found to occur in an artery relaxed in all its dimensions, and more or less imperfectly filled. It is by no means necessary to produce such tracings as those shown that the aortic valves must be incompetent, it is sufficient that the arteries from any cause be relaxed and less full than usual; hence, in old age, when these conditions frequently exist in a considerable degree, the pulse tracing, even when the aortic valves are perfectly healthy, may be undistinguishable from that of aortic regurgitation.[1] The pulse-wave consists of two parts—a percussion wave transmitted by the arterial walls, and a pressure or propulsion wave of blood which passes within the arterial lumen. In most pulse tracings the effect of both of these is conjoined in producing the abrupt line of ascent with which every pulse trace commences, and which is large and pronounced in proportion to the force of the beat and the size of the blood-wave; most developed, therefore, when the left ventricle is hypertrophied and dilated from any cause. When, however, there is disproportionate delay of the pressure wave, as happens when the arteries are unfilled, which are, of course, longer than usual of reaching their maximum distention, then exactly in proportion to the difference of the rate of propagation of the percussion and the pressure wave we have developed a tendency to the formation of a beak, or, as the French observers term it, a *crochet*, at the summit of the line of ascent. This beak is due to the vibration of the arterial walls out-stripping the blood-wave, and it may be increased, or more fully brought out by taking the pulse tracing with the arm elevated;[1] because, although in aortic regurgitation both kinds of waves are delayed as a loose string vibrates more slowly and with larger undulations than one more tense, precisely as an unfilled elastic tube is longer of reaching its maximum distention than one more nearly distended—yet by the elevation of the arm the gravitation of the blood tends to delay the arrival of the maximum of the pressure wave at its upper end, while no additional obstacle is opposed to the advance of the percussion wave. When the upward and downward lines of the beak lie in the same plane, as in the upmost of the two tracings given, our reasoning in regard to the mode of its formation is open to the objection that it may be due to the imperfection of the instrument, the initial impulse of the vibration wave having been so great as to have jerked the writing lever above the influence of the pressure wave, down to the level of which it must fall—this fall being instantaneous—before its influence can be felt and perceived in the gradual separation of the downward from the upward line until it culminates in the summit level of the pressure wave. In like manner, we might suppose that the more distinct separation of the downward from the upward line of the beak, which is visible in the undermost of the two pulse tracings given, might possibly be due to the more rapid movement of the writing tablet in the latter than in the former case, and that, therefore, the writing lever, unduly jerked up by the vibration wave, had time in the one case to return perpendicularly on its own trace; while in the other the more rapid movement of the tablet gave its descending line a slanting direction sufficient to separate it slightly, as is the case, from its ascending trace, and thus to give it only artificially the appearance as if the vibration wave was never actually, but only relatively, separated from the pressure wave. But when we know that both of these tracings were taken by the same instrument, on the same tablet, moving at the same rate, the conclusion seems inevitable that in the one case there was a much greater obstacle to the advance of the pressure wave than in the other—that is, that the regurgitation was greater in the one case than in the other, which is precisely what really existed (*vide* Cases V. and VI.). Corroborative proof of the truth of this statement is to be found in the fact that, whenever by increasing the pressure on the artery we destroy more or less completely the influence of the percussion wave, thus compelling the pulse tracing to be solely formed by the pressure wave; then, whatever may be the nature of the pulse, we never have a beak formed, the pulse tracing has always a rounded summit proportionate to the degree in which the pressure has destroyed the influence of the percussion wave, and has even deformed that of the pressure wave—the latter deformity being sometimes so great as completely to efface all trace of the natural dicrotism of the pulse. Of this fact few series of pulse-tracings are without notable instances,[1] and the following tracings from a different disease afford a very well marked example:—

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[1] Marey, Physiologie Médicale de Circulation du Sang, Paris, 1863, p. 419, etc.

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[1] Loraine sur le Pouls, Paris, 1870, p. 258, etc.

FIG. 3.

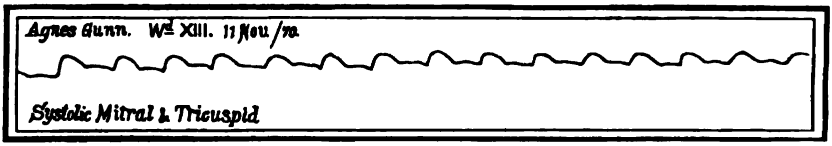
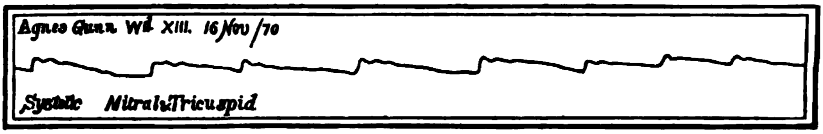


FIG. 4.



The pulse in the one tracing is much faster than in the other; that, however, is of little consequence, the rounded summit of the one will be found to rise to a less height than the sharper peak of the uncompressed artery, and though the pulse was so feeble as to give when uncompressed but a faint tracing of the natural dicrotism (almost filiform), this will be found to be entirely effaced when excessive pressure has modified the tracing. Sphygmographic tracings of the pulse are very advantageous as revealing the local succession of events in the artery; they cannot be depended upon, however, as an index of the relation of these events to the cardiac rhythm. To determine this we must call in the aid of electricity, which has not yet been done so thoroughly as might be. Further, from our practical experience and reasonings on the mode of formation of these pulse tracings, we can usefully employ them for many purposes, but inasmuch as by no form of sphygmograph yet invented can we ensure that the pressure upon the artery shall be always the same, such tracings are of little practical value, except in so far as they indicate the regularity or the irregularity of the pulse.

The comparative longevity and comparative frequency of sudden deaths in aortic and mitral incompetence have been the subject of much debate and of much variance of opinion. In regard to the comparative longevity, accurate data, involving the life history of many such patients, are unquestionably wanting, and we can only fall back with certainty upon hospital experience; and this enables us to say that in regard to those affected with aortic incompetence from disease, who come under treatment on account of serious disturbance of compensation, the case is very much the same as with those suffering from aortic incompetence the result of traumatic rupture—four years, reckoned from the date of admission, will include the day of death in by far the larger proportion; while this is by no

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[1] *Vide* Marey and Loraine, *passim*.

means the case in mitral incompetence. We must remember, however, that many of these cases of aortic incompetence have, from their history, evidently laboured for very many years under their valvular defect, which has only at last asserted itself as a disease, when from defective nutrition of the cardiac walls, into the causes of which I have already entered, efficient compensation could be no longer maintained. This hypothetical persistance of aortic incompetence, mute for many years, is confirmed by those comparatively few cases in which compensation, accidentally ruptured, has been restored by efficient treatment, and has remained perfect for long, certainly for several years. It is also confirmed by those cases of not infrequent occurrence in private practice, and sometimes met with in hospital, in which a lesion of the aortic valves is detected, which is perfectly mute, the past and future duration of which are alike unknown. On the other hand, very similar statements may be made in regard to cases of mitral incompetence. To this we have to add the well-known fact, that aortic incompetence is always the result of a valvular lesion which can only be rehabilitated in the rarest instances, and then only imperfectly; while mitral incompetence is very commonly the result of perfectly curable disease.[1] A due consideration of all these facts enables us to say certainly that though as yet we have no sufficient data to enable us to decide as to the comparative longevity in aortic and mitral incompetence; yet that, inasmuch as the former is always accompanied by serious valvular lesion, while the latter is not, the probability of life is very greatly in favour of a mitral lesion. Further, though we cannot estimate with certainty the value of life in any given case of aortic lesion, we may with tolerable accuracy fix the limits of vitality as lying within three months in cases of unfavourable rupture of the aortic valves, and twenty-five years in cases of disease occurring under favourable circumstances, which may vary much in character; while within these limits our prognosis must be determined entirely by the circumstances of the patient, including the necessity or not of manual exertion for procuring his livelihood, the many prospective conditions which may influence the nutrition of his body, among which his habits—temperate or otherwise, etc.—are of the greatest importance, as well as the past history of his family, as affording a probable clue to what these habits may become; and, lastly, the family longevity of the patient, which affords an indication of the greater or less tendency to muscular degeneration—an indication which can never be wholly vitiated, though it may be much modified by the accidental position of the coronary arteries, or the greater or less destruction of the corresponding segments of the aortic semilunar valve—facts which can never be definitely ascertained before death. Further, the length of time during which the disease has probably already existed, and the present condition of the cardiac muscle, form most important elements in the prognosis. Nor must we forget, especially in the early stages of the disease, to give due prominence to the temperament of the patient, nervous excitability, whether evinced in sudden bursts of passion, or in a tendency to irregular spurts of violent exercise, being specially unfavourable—whatever conduces to irregular action of the heart being injurious in all forms of cardiac disease, but infinitely more frequently fatal in aortic incompetence than in any other. And this leads me to consider that other *quæstio vexata*, the cause of death in aortic regurgitation, and especially the frequency of sudden death in this form of cardiac lesion, compared with others. Death, whether it arises from the loosening of the silver cord or the breaking of the golden bowl, is always a momentary and therefore sudden act: but we only so regard it when it appears sudden or unexpected relative to the apparent previous condition of the patient. When an individual, therefore, drops dead upon the street, or in a public meeting, or is found dead in his bed after retiring to rest in his usual health, he is popularly said to have died suddenly, though the sword of Damocles, in the shape of an inevitably fatal disease, has for years hung over his head ready to drop at any moment, and though his death relative to his actual state of health has been by no means so sudden as that of another hurried off by a fever or other acute disease of a few weeks' or days' duration. From this, the popular point of view, it is by no means difficult to say which of all the cardiac lesions is most likely to prove suddenly fatal. Death from any cardiac lesion occurs from syncope, and this is brought about in two ways—first, by gradually increasing asthenia, in which the aortic blood-pressure slowly fails from day to day, till at last it drops below what is compatible with life, and death ensues; second, by asystole, in which the aortic blood-pressure suddenly falls below that necessary for the maintenance of life, because the left ventricle ceases to act. No statistics are required to prove to us which of these accidents is most likely to happen in any given cardiac lesion.

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[1] As in chlorosis and other forms of anæmia, etc.

When the muscular walls are enfeebled from inflammatory infiltration, fatty or fibrous degeneration, they may suddenly cease to act at any moment, either with the heart in systole or diastole—more often the latter—and with this cessation of action life comes to an end. Death from this cause may occur with or without the presence of valvular lesion; if the former, there are, as we have seen, both anatomical and physiological reasons why it should be incomparably more frequent in connexion with aortic incompetence than with any other valvular lesion. In the most extreme form of mitral constriction, enough of blood always reaches the ventricle to enable life to be maintained, if only in a feeble fashion, while no obstacle is presented to the free action of the ventricle; in the greatest mitral regurgitation there is no want of blood in the ventricle, and provided the contraction of that organ is powerful enough, a sufficient quantity is always sent on to provide for the maintenance of life; and as the blood has in such cases always two ways of escape, ventricular systole, though it may be difficult, is always possible, except in the presence of muscular degeneration. In these two forms of disease, therefore, sudden death from asystole must be and is a somewhat rare affection, the more usual mode of death being syncope from asthenia, brought about by secondary diseases, such as pulmonary apoplexy, or more slowly by dropsy, with or without jaundice or albuminuria; or suffocation suddenly induced by pulmonary œdema. On the other hand, in aortic incompetence the constant and continually increasing dilating pressure of the blood column acts as a permanent obstacle to ventricular contraction; and we can readily understand why comparatively trifling exertions, which tend to increase the aortic blood-pressure, and also why emotional excitement, which interferes with the nervous supply of the ventricle, and consequently with its regular action, should in very many cases be instantly followed by sudden death. The pause of a second, even with a comparatively healthy ventricle, permits the dilating power, already barely compensated, to turn the scale, and after a few feeble attempts to overcome the obstacle,—the primary syncope passes into death from asystole, the heart remaining in a permanent diastole.[1] And if this be the case, even with a comparatively healthy ventricle, how much more likely is this to take place when its muscular tissue is diseased, as is so frequently the case. When the mitral valve becomes incompetent from secondary dilatation, death from asystole is not so readily brought about, but it is not altogether prevented. The cavity which must then be filled to overflowing is no longer represented by the ventricle alone, but includes also the lungs, the terminus being shifted from the mitral to the pulmonary valve. When in such circumstances the pulmonary capillaries are fully congested, any accidental interference with the regularity of ventricular action must be followed by asystole, or by artificial relief within the closed circuit, rupture of the heart or pulmonary capillaries, the latter being the *pars minoris resistentiæ* we have next to asystole pulmonary apoplexy as the most frequent cause of death in aortic incompetence. From the mechanical cause, always present in aortic insufficiency and always acting with increasing efficiency in the production of these two modes of death, and from the readiness with which the pulmonary capillaries may be ruptured, we can easily understand that those cases of aortic incompetence which escape death from asystole, mostly die from pulmonary apoplexy, or from a combination of the two; and it is only comparatively rarely that dropsy and other secondary diseases of a serious character are established in such cases, death then occurring from asthenia. This you will see is entirely a question of animal mechanics, which statistics may confirm, but can never disprove. The mode of production and morbid appearances after death from asystole are precisely the same, whether that death has been on the street, in a public meeting, in the midst of apparent health, or after a longer or shorter period of more evident illness. In illustration of it I therefore prefer to give two cases in which the disease was known to have lasted for some time, because their history has an important bearing on the treatment of such cases, to which I shall immediately refer.

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[1] This subject has been very fully and carefully investigated by Dr Maurice. *Vide* his Essai sur les Maladies de Cœur. De la Morte Subite dans l'Insuffisance de l'Aorte. Paris, 1860.

CASE V.—Isabella Stewart, aged 35, employed at paper mills, admitted on 30th May 1870, to Ward XIII., complaining of cough, which troubled her for the first time about a fortnight ago, and palpitation of the heart, from which she had suffered about eighteen months ago. Her previous health was good. Family history unimportant. Her expression was anxious, muscularity flabby, skin dusky, but naturally so; lower extremities slightly œdematous, joints normal, has never had rheumatism. Pulse 93, presenting in a well-marked form all the phenomena of the Corrigan or water-hammer pulse. On inspection and palpation the area of cardiac impulse was found to be somewhat extended—the impulse itself, however, being somewhat feeble. There was visible pulsation in all the superficial arteries, particularly in those at the root of the neck, but by no means well marked. The apex of the heart was found to beat under cover of the sixth rib at a distance of three inches and a half from the left edge of the sternum. At one inch from the left edge of the sternum, dulness commenced at the level of the upper edge of the third rib, and extended downward to the liver dulness. Transversely, at the level of the fourth rib, dulness commenced half an inch to the right of the sternum, and extended across for a distance of five inches. On auscultation over the apex, the first sound was found to be much obscured by coarse crepitation, and also by a murmur, which, on being traced upwards, was found to have its point of maximum intensity at mid-sternum between the third and fourth ribs; the second sound is wholly replaced by a soft diastolic murmur—this double murmur is propagated into the arteries, the diastolic portion only faintly; it is also propagated with greater or less distinctness over the entire cardiac area. In the pulmonary region the pulmonic second sound is much obscured by the diastolic murmur, but on moving the stethoscope wholly off the sternum it is distinctly audible about an inch to the left of its usual position. Respirations 24; cough troublesome; sputa thin, watery, and somewhat frothy, amounting to three ounces in two hours. Pulmonary percussion normal. On auscultation, the respiration was entirely obscured by crepitation, cooing and sonorous rhonchi. Appetite gone; some thirst; urinary and other systems normal. Diagnosis, acute bronchitis, aggravated by the œdematous and congested conditions of her lungs, the result of aortic incompetence. Prognosis, grave, death being threatened by cardiac asystole. Treatment—full diet, six ounces of wine, dry cupping to the chest, with large jacket poultice afterwards, and the following prescription:—

℞ Ammoniæ carbonatis	ʒi.
Tincturæ hyoscyami	ʒiv.
Potassii iodidi	ʒi.
Tincturæ digitalis	ʒi.
Infusum calumbæ *ad*	℥vi.

Sig.—One tablespoonful every four hours.

Up to 15th June, it is noted that the patient's condition had been repeatedly aggravated by attacks of pulmonary congestion and threatened asystole, from which she had been relieved by dry cupping, stimulants, and the use of the mixture prescribed. To-day there is almost no cough, expectoration nearly gone, vesicular respiration everywhere audible, pulse 88, full and strong; patient is lying quietly, and expresses herself as quite relieved. On 18th June, she was up at visit, and apparently quite well; but on the 22d, she had another attack of congestion, with threatened asystole, and in spite of renewed efforts, by which she was temporarily relieved, she died suddenly on 26th June. At the post-mortem examination of the body, which took place fifty-six hours after death, the heart was found more dilated than hypertrophied—it weighed 19 oz.; both ventricles were filled with dark-coloured clots, of which that on the left side extended some distance up the aorta, presenting one continuous mould of the interior of the left ventricle and of the aorta. The aorta itself was slightly dilated, its valves thickened and incompetent, a considerable angular aperture being left between the edges of its cusps. Pulmonary, mitral, and tricuspid valves competent and healthy. A portion of the muscular substance of the heart was somewhat inflamed.[1] The right lung was healthy both in appearance and on section, except at its lower margin, where there was a patch of pulmonary apoplexy. Pulmonic glands infiltrated with black pigment, and a good deal of pigment on the surface of the lung. Left lung adherent all round, especially posteriorly, by recent lymph, in some places quite one-fourth of an inch in thickness. The lung itself is œdematous, and the bronchial mucous membrane congested. Iodine produced a slight mahogany tint of the liver, which was fatty, and presented a nutmeg appearance. Spleen healthy. Kidneys congested, but otherwise healthy; Malpighian bodies slightly prominent, but presented no reaction from iodine.

This case, then, presents a very well marked example of death from asystole, in which compensation had never been fully developed, but in which the death was certainly hastened by inflammatory complications.

In the following case the asystole was the result of long-continued disease, which had been fully compensated, but the compensation of which was accidentally ruptured. Even in his case, therefore, death was a preventible accident, though the unavoidable end was not long forestalled.

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[1] This is all that is recorded in this respect in the Pathological Records. I shall subsequently have occasion to point out the nature of this myocarditis and the influence it has in promoting asystole.

CASE VI.—William Yorkston, a blacksmith, aged 54, admitted to Ward V., 14th November 1870, complaining of orthopnœa, cough, expectoration, occasional pain in the chest, and swelling of the legs. About six weeks ago he caught cold after prolonged exposure to heat at his work, and this cold was aggravated by a fresh exposure about a fortnight ago. Has always enjoyed good health, has never had rheumatism; but for the last three or four years has been drinking heavily, and working hard. His father and mother both died of cholera. The patient was tall and muscular, his expression anxious, his face pale, his lips livid, his skin and joints normal, his legs, from the knees downwards, œdematous. Radial pulse jerks slightly, but in by no means a marked manner, beats 90 per minute. The jugular veins pulsate visibly, the right carotid slightly, and the left not at all. The cardiac impulse is diffused and feeble, the apex beating between the sixth and seventh ribs and diffusedly. Percussion dulness at one inch to the left of the sternum commences at the upper border of the third rib, and extends down to the liver dulness. Transversely on the line of the fourth rib, dulness commences half an inch to the right of the sternum, and extends across for a distance of five inches and a half. In the mitral area a murmur is heard to replace the first sound, but the second sound is audible. On tracing the systolic murmur upwards along a line joining the mitral and aortic areas, it is found to get gradually fainter on nearing the sternum, and again gradually to increase in loudness till it reaches its second maximum over the sternal end of the second rib on the right side; in this position the second sound is entirely replaced by a loud but soft diastolic murmur, the systolic portion of the murmur alone is propagated into the carotids. In the pulmonary region a second sound is audible, but, obscured by the diastolic murmur, it becomes more distinct on moving the stethoscope along the second interspace till its edge is quite off the sternum. At the lower end of the sternum the systolic murmur is very loud and distinct, and wholly replaces the normal sound of the tricuspid. Respirations 26 per minute; cough hard and frequent; sputum watery, frothy, and small in quantity, no blood. Percussion of lungs normal. On auscultation, sonorous and sibilant rhonchi are heard over the chest, partly obscuring the vesicular breathing. Appetite defective, bowels costive, urine scanty, no albumen. Patient sleeps none at night. He was ordered full diet and four ounces of whisky daily.

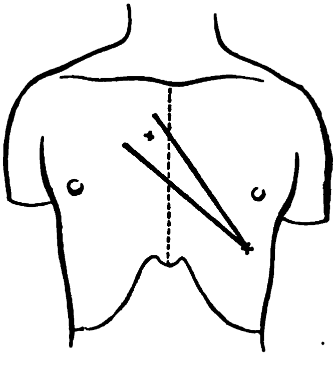
℞ Tinct. scillæ,
„ digitalis, āā ʒii.
Aquæ cassiæ, ℥v.ss. M.
Sig.—One teaspoonful every four hours.

℞ Elaterii gr. ⅛. Ft. pil.—One such to be taken every four hours till the bowels are freely moved. To have twenty grains of chloral every night at bed-time.

On 21st Nov. it is noted, patient slept last night in the recumbent posture for the first time. His dyspnœa is now comparatively trifling, the cough gone, the respiration free, the œdema of the legs entirely gone, his cardiac impulse stronger, the pulse more markedly jerking, only 80 per minute; it is delayed the full half of a cardiac revolution beyond the apex beat; the superficial arteries pulsate now in a marked manner, the jugular veins less distinctly. Over the mitral and tricuspid areas the systolic murmur is not so loud as formerly, and much less distinctly localized. He was kept some time under observation, and then discharged much improved. On the 18th of August 1871, he was re-admitted, and died suddenly next day. At the post-mortem examination the body was well developed; skin and conjunctivæ of a yellowish colour. The heart was greatly enlarged, dilated, and hypertrophied, weighing thirty ounces. Both ventricles contained large black clots. The aortic valves were atheromatous and incompetent, the aorta itself greatly dilated. The mitral and tricuspid valves both admitted five fingers. The right lung was very dark in colour, greatly congested, and apoplectic; it weighed two pounds two ounces. The left lung also contained an apoplectic clot, and was covered with recent lymph. The liver weighed three pounds, was fatty, and its ducts congested and obstructed by catarrhal mucus. Kidneys enlarged and congested.

These two cases illustrate very well the condition of the heart which is found after death from asystole, both ventricles, but especially the left one, being dilated and filled with a black clot extending up into the aorta, death occurring from a paralysis of the ventricular muscle, brought about by over-distention. That is the mode in which death in aortic incompetence primarily invades the heart. As we have seen, it may be brought about in many ways. Our object is to prevent it, and in obviating it we shall also in a great measure be able to prevent many of the secondary causes of death, or at all events delay their fatal result.

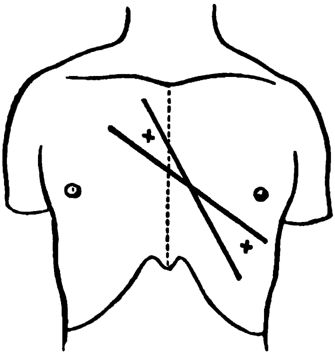
FIG. 5.



In Case V. it is noted that the first sound over the mitral area was obscured by a systolic murmur, which had its sole focus of maximum intensity in the aortic area. This was ascertained by tracing the murmur upwards along a line joining the mitral and aortic areas, and the results are graphically shown in the accompanying diagram (Fig. 5).

In Case VI. the systolic murmur is stated to have two foci of maximum intensity; that is, that in tracing it upwards from the mitral to the aortic area, it was found first to decrease in intensity and again to increase, as shown in the accompanying diagram (Fig. 6).

FIG. 6.



This is the method to be pursued in attempting to determine whether we have to do with a single systolic murmur of mitral or aortic origin, or with a double murmur originating at both orifices; and these diagrams represent graphically the results we obtain in tracing the murmur from one orifice to the other in two of three hypothetical cases. Of course, when the systolic murmur is purely of mitral origin, the graphic representation would be exactly the reverse of that depicted in Diagram 5.

In aortic incompetence we have seen that the most injurious effect is produced by the dilating force of the column of arterial blood, which, according to Pascal's law, acts in proportion to its height and the area of its basis, and our primary object is to reduce as far as possible both of these elements. First of all, therefore, we put the patient to bed, and endeavour to get him as nearly recumbent as possible, so as to diminish the height, and consequently the distending power, of the arterial column. With this view we also deaden his sensibility by the administration of chloroform, chloral, or morphia, either by the mouth or subcutaneously. The orthopnœa, which is so distressing a feature of this disease, has reference solely to the congested and œdematous condition of the lungs, and is established by nature to give the patient's respiratory muscles a better purchase in elevating the chest walls. A wise physician knows, that however dangerous the condition of the respiration may be, the patient runs much more risk from the state of his heart. He does not argue the point with nature, but he calms her instinctive fears with the means at his command, and he feels that he has gained a victory for science when he has got the patient fairly recumbent. The inexorable mechanics of the circulation are then appeased, and we have leisure to attend to the physiological part of the difficulty. So intricately involved, however, are all our vital actions, that as we cannot appease the mechanical part of the difficulty without advantage to the physiological part, so also we cannot remedy the physiological portion without also benefiting the mechanical portion. It is advantageous, therefore, to carry out both parts of our treatment at once; and though to the superficial thinker such a plan has the appearance of an empirical treatment of symptoms, a deeper knowledge of mechanics and of physiology, and of their mutual interdependence, will show that we are really employing scientific means according to scientific method, and that we apparently thwart nature only the better to secure the ends she aims at; thus conclusively proving the superiority of scientific art to empirical nature. While we therefore attempt, by getting the patient down, to lower the height of the distending column, thus at once relieving the cardiac circulation and diminishing the pulmonary congestion, we simultaneously endeavour to produce a similar relief by diminishing the area of the base of the arterial column, and this, so far as our present therapeutical armamentarium extends, we can only do by means of one drug—digitalis. It is not so long since this drug was regarded as a pure sedative to the heart—the opium of the heart, it was euphemistically termed. Now, thanks to experimental physiology, we certainly know that its action is widely different from that of opium, and greatly superior to it so far as preservation of life in this disease is concerned.[1] It is still regarded by many as not merely useless, but positively dangerous, in the disease of which I speak,—aortic regurgitation;[2] and yet there is no other disease in which this drug is of more value, and no other in which its curative action can be more efficiently demonstrated than in this. In very large doses digitalis is employed as a sedative in many diseases, such as delirium tremens, pneumonia, etc., but experimental physiology has shown us that such a method of inducing what our American cousins call sedation is an extremely dangerous one, as it depends upon the fact that the stimulating blood-supply is cut off by an extreme degree of ventricular contraction, which falls just short of the ordinary fatal result of digitalis poisoning—death with the heart in systole. A full dose of digitalis maintains, however, its sedative action for some time without by any means proving fatal. The almost fatally contracted condition of the ventricle is not therefore the result merely of one or two energetic contractions which again relax, but is the result of a tonic contraction of the ventricle, which is excited only to a certain pitch by a certain dose of the drug, and beyond this it does not pass unless the dose is increased, nor does it greatly relax for some time, till the effect of the drug passes off. The ordinary employment of digitalis teaches us the same thing. If we gradually increase the dose at regular intervals, or if, employing a large dose at first, we persist in its use, similar phenomena occur. First we have a gradually increasing fulness and firmness of the pulse-beat and of the ventricular systole, and then a falling off of both; smallness of pulse, feebleness of heart-beat, irregularity, and, finally, fatal syncope with the heart in systole. There is a regular gradation between these two extremes. So long as we employ small doses we may go on administering them daily for years—as is often done in mitral disease—without any fear of untoward result, without any dread of cumulative action. But the instant we employ large and powerful doses, the difficulty of regulating the action of the drug becomes extreme, and we require to watch its action very carefully, and suspend it on the very earliest indication of its poisonous action. The useful employment of digitalis in aortic incompetence is purely a question of dosage; in a few months or a few years we shall be able to regulate it with much greater nicety than at present, with more comfort to ourselves and more safety to our patient; at present we must risk something for the sake of an otherwise unattainable benefit. What we desire to produce in each case is just such an amount of tonic contraction of the ventricle as shall rather more than counterbalance the dilating power of the arterial column. If our patient is obliged to be upright, a larger dose will be required than if we can lay him flat; and the larger the dose required, the more carefully it must be watched. The dose must be accommodated to the circumstances of the patient, and regulated by them, as well as by his idiosyncrasy—for some are more susceptible to the action of the drug than others. In the employment of a drug where accurate dosage is of so much consequence as in this, it is of importance to have a preparation of uniform strength, and in this respect I have found the ordinary tincture all that can be desired. The ordinary infusion is not so uniform, nor is the crystallized digitaline so reliable. The latter is very convenient for subcutaneous injection when rapid and immediate action is necessary, but it cannot be so certainly relied upon as the tincture. In the cases already related, the doses of the tincture have varied from five to ten minims every four hours; but I have frequently doubled or tripled these doses, and sometimes even gone beyond that with benefit—in fact, though commencing, as you have seen, with small doses (five minims) of this drug, and trusting somewhat to other tonics, I now trust mainly to digitalis, giving fifteen minims up to half a drachm of the tincture every four hours; and in one very remarkable case of persistent threatening of complete asystole I was only enabled to get the patient out of the Infirmary and sent home, a distance of a hundred miles, by the continuous use of half-drachm doses of tincture of digitalis every two hours for several days.[1] The use of digitalis is always accompanied by greater or less increase of the flow of urine. So long as this keeps up we are—I speak from experience—quite safe to continue its use. When employing moderate doses of digitalis, the flow of urine may halt or diminish without fear of untoward result; but in using large doses we must look upon the mere halting of the flow of urine as an indication to us to watch the pulse with great care several times a day, and if at any time it commences to thump or to falter, or if nausea be induced on movement, we ought at once to pretermit the use of the drug. With this precaution I have never found the use of the drug in this disease, even in these large doses, productive of anything but relief, a relief unattainable by any other means I know of. Tincture of the perchloride of iron is in many cases a useful adjunct, as it helps to improve the blood and assist in the nourishment of the cardiac muscle; but now and then it disturbs the stomach, and must be omitted. It never is of so much consequence as digitalis, and a good supply of animal food may fairly enough replace it. The liquor arsenicalis stands in a different category; it is not only a hæmic tonic, but it is a special tonic to the cardiac muscle, and in moderate doses rarely disturbs digestion. In aortic incompetence the compression to which the ventricular muscle and its vessels are subjected give rise, as we have seen, to mal-nutrition. Neuralgia is said to be the prayer of the nerves for better blood; hence neuralgic pain is a frequent accompaniment of this disease. In all forms of cardiac angina arsenic is almost a specific, and in this form it certainly acts with great benefit. At present we can only employ it empirically; but it is not so long since digitalis was only employed for a similar reason, and we confidently look forward to a time when the use of arsenic in cardiac neuralgia shall be justified by accurate physiological reasons. Of course it is occasionally advisable to conjoin the use of these remedies with that of others possessing certain other specific actions, such as diuretics, purgatives, etc.; but these are to be used *pro re nata*, with reference to the individual case, and not to the disease generally. All such patients require to be nutritiously yet moderately fed, because the defective aortic pressure reacts injuriously on both the gastric and hepatic secretions, and limits both their supply and their efficiency. Ordinary alcoholic stimulants are of great use in such cases, moderately supplied, but their employment must be watched and regulated. They are only of temporary value to tide over a weakly period, and by no means possess the permanent value of such special cardiac stimulants as digitalis, arsenic, or even iron.

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[1] For very many years I have been in the habit of employing digitalis as a cardiac stimulant, but it is only since the results obtained by experimental physiologists have been published, that I have felt justified in using it with that freedom necessary to secure its most brilliant results.—*Vide* Ed. Med. Jour., Feb. 1870, p. 743.

[2] By none more definitely than by Dr Fothergill, whose work on digitalis has done more to popularize the scientific use of that drug than any other I am acquainted with.—*Vide* On Digitalis, London, 1871, p. 52.

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[1] Agnes Butler, admitted to Ward XIII. 19th October, discharged 16th November 1872.

Part Second.

REVIEWS.

Autobiography of A. B. Granville, M.D., F.R.S. Edited by his Daughter, PAULINA B. GRANVILLE. Two volumes 8vo. London: Henry S. King and Co.: 1874.

WE regard this as a remarkable book in more ways than one.

The author, an Italian by birth, and grown up to man's estate before coming to England, writes the English language—as few natives can do,—in an easy, fluent, often eloquent, style—without mannerism, without foreign idiom, without extravagance of any kind. His powers of graphic description are very great; and he enjoyed unwonted opportunities of observing human nature in many parts of the world, in all ranks of life, and under circumstances exceptionally varied and peculiar. Few persons have come across so many men and women of pre-eminent ability, and fewer have possessed the intellectual and social qualities which fitted them for securing the confidence and friendship of those whom they happened to meet. Dr Granville's preliminary education was very complete, both in classics and in science. He studied medicine under such men as Antonio Scarpa, Spallanzani, Rasori, Brera, and Volta. He had an unusual aptitude for the acquisition of languages, and wherever he went was speedily at home with the natives, whether in Greece, or Asia Minor, in Spain, or France, or England; and these linguistic attainments gained ready admission for him as an agreeable and useful member of society, and on many occasions rendered him of special service in the harmonious settlement of difficult and delicate political emergencies. Not a few occurrences and coincidents of his eventful career will almost suggest to some readers the history of Baron Munchausen; but the Doctor's veracity and trustworthiness are beyond suspicion, and all the more startling statements in his biography are invariably supported by day and date and document.

It is a remarkable book for other reasons. Among these, we may specify that it was written, or at least arranged for publication, and that very admirably, during the two years which preceded his death, at the ripe age of 88.

Few men have lived so *intensely*, and few have made so many friends; and yet it is a curious fact, that his name will sound unfamiliar to most of the present generation. Dr Granville was born in the same year (1783) with William Lawrence and Benjamin C. Brodie, and five years before Henry Holland, whose "Reminiscences," although interesting, and in some points analogous, are less satisfying than the "Autobiography."

A Roman Catholic in his early years, he lapsed during his youth into atheistic views, but ultimately embraced the Protestant religion; and, in his declining years, seemed to trust with child-like simplicity in the Divine and Personal Saviour.

Our limited space prevents our quoting any passages, but we very cordially recommend the book as one of no ordinary interest, and very suggestive to readers of intelligence and liberal education.

The Histology and Histo-Chemistry of Man: A Treatise on the Elements of Composition and Structure of the Human Body. By HEINRICH FREY, Professor of Medicine in Zurich. Translated from the Fourth German Edition by ARTHUR E. J. BARKER, Surgeon to the City of Dublin Hospital, etc.; and Revised by the Author. With 608 Engravings on Wood. Pp. 683, 8vo. London: J. and A. Churchill.

THIS work must be regarded as the best advanced text-book on human histology and histo-chemistry in the English language. After a brief historical introduction, the work is divided into three parts: 1. The elements of composition and of structure of the human body; 2. The structure of the tissues of the body; and, 3. The structure of organs.

The most of the first part is occupied with a discussion of what is termed histo-chemistry. This branch of knowledge treats, not only of the chemical constitution of the structural elements and of the tissues, but also of the metamorphosis, decomposition, and elimination of all substances occurring in the tissues during their life. A knowledge of histo-chemistry is the complement to histology. Science in recent times has advanced with greater rapidity towards a knowledge of form than of chemical composition, but the labours of such men as Mulder, Donders, Hoppe-Seyler, Kühne, and Schlossberger, have done much to remove some of the deficiencies of this branch of study. Frey assumes that such substances as albumen, myosin, and gelatin, exist as such in living textures. We must not forget, however, that of this there is no proof. Living textures give none of the reactions characteristic of these substances; they do so only on the advent of death. Still, for practical purposes, the assumption is useful. Frey points out the remarkable instability of the protein compounds, and suggests that they may readily be converted into the ferments present in various secretions. These ferments in turn, as is well known, excite chemical changes in albuminous substances, starches, and neutral fats; and thus the transformation of one of the most important substances in the body introduces a new series of chemical actions. We observe that gelatin is described under the names of collagen (or ordinary glue) and gluten. The description of the fats is meagre in detail. Under the title of cerebral matters, two substances are described, namely, cerebrin and lecithin, and it is asserted that the substance protagon, described some years ago by Liebreich as existing in nervous tissue and in the yolk of hens' eggs, is simply a mixture of these two substances. Urea, creatin, etc., are described under the heading of Amides. On this theory, urea is regarded as being constructed upon the type of two atoms of ammonia in which two of the hydrogens are replaced by the diatomic radicle C O. But urea, though it exhibits most of the characters of an amide, especially on being resolved into carbonic acid and ammonia by the actions of acids and alkaloids, nevertheless does not appear to react like an amide under all circumstances, as was pointed out by Wanklyn and Gamgee. Under the head of animal colouring matters, the various products of hæmoglobin—namely, hæmatin, hæmin, and hæmatoidin—are described, and it is correctly pointed out that the second of these, hæmin, is really the hydrochlorate of hæmatin.

The next part of the work treats of elements of structure, starting with the cell. According to the author the cell is to be regarded as the first anatomical and physiological unit, but he admits that other particles, smaller than the cell, and existing before it, may be met with. He is obliged to change the definition of a cell given by Schleiden and Schwann, and to adopt the observation of Häckel, that a minute particle of protoplasm suffices for all the requirements of the lowest grade of life. It will thus be seen that the cell of modern histologists is not the cell of Schleiden and Schwann. There can be no doubt that the tendency of modern research is to locate the first manifestations of life, not in the cell, but in fragments of protoplasm, which are similar in many respects to the molecules contended for by Hughes Bennett. The phenomena of vitality observed in cells, such as the colourless blood corpuscle, are described as absorption of matter, transformation and excretion of matter, growth, proliferation, and contractility. The life of the individual cell is carefully delineated, and it is shown that the living cell manifests a transmutation of the matters of which it is composed. Here we have the first physiological process, a knowledge of which in its simplest form must be the key to processes occurring in tissues of the highest physiological dignity, such as nerve and muscle. This part of the work is extremely interesting, and must be carefully studied by all who wish to appreciate the direction in which pathological and therapeutical knowledge is advancing in the present day.

Frey divides the tissues into five groups, namely:—1st, Tissues composed of simple cells with fluid intercellular substance, such as blood, lymph, and chyle; 2d, Tissues composed of simple cells with a small amount of solid intercellular substance such as epithelium and nail; 3d, Cellular tissues, usually described as the connective tissue group, such as cartilage, colloid, adipose, and connective tissue, bone, and dentine; 4th, Tissues composed of transformed non-cohering cells, such as enamel, lens, and muscle; and, 5th, Compound tissues, such as nerve, gland, vessels, and hairs. The author then proceeds to describe the histological structure of the various organs.

No one can read or even glance through this volume without being struck with the rapid advances made from month to month in histology. At present, by the employment of new methods, there is such a vast accumulation of facts, as almost to bewilder the earnest student. Tissues that some years ago were supposed to be simple are now found to be complex, and processes which recently were supposed to be well understood are now shown to have been erroneously described. For example, we must not now speak of the transformation of cartilage into bone, but of the substitution of bone for cartilage. The description of this process is given by Frey with greater lucidity than by any other writer we have met with.

We can strongly recommend the work to advanced students. It is not so ponderous as many German works, and there are manifestations in it of the logical arrangement characteristic of a French mind. The woodcuts are, on the whole, good, but some have given too dark an impression to resemble the true appearance of the tissue seen under the microscope.

In conclusion, it must be evident that, having regard to the rapid advance of histology and the necessity for a division of labour, chairs or lectureships on this subject must be founded in our Universities as in France and Germany. At present the labour is divided between the professors of anatomy and physiology. Either of these subjects is extensive enough to demand the entire energies of one man, and there is ample room for a third in the intermediate territory of histology.

Virchow's Archives, Vol. LXI. page 253. "*Experimental Researches regarding 'the Waxy Degeneration of Striped Muscular Fibre.'*" By Dr W. WEIHL of Thal-Itter.

To an English reader the title of this paper might be somewhat misleading, the term "waxy degeneration" being associated in our minds with a well-known diseased condition, recognisable by its characteristic appearances, and by the use of the familiar iodine test.

The condition with which we are at present concerned is entirely a different one. Other titles have been given to it which are less open to objection, and quite as descriptive as the above. It has been called, for example, "the colloid degeneration of muscle" by Uhle and Wagner; the "glassy degeneration" (dégénérescence vitreuse) by Cornil and Ranvier; the "flaky splitting" by Neumann; and, lastly, it has been sometimes termed, in honour of the observer who brought it prominently into notice, "Zenker's degeneration of muscle." Perhaps of all these titles, "the glassy degeneration" is the best, as it is descriptive of the morbid appearance, and involves no theory regarding its nature.

The glassy degeneration of muscular fibre was first described by Dr Bowman, in the Philosophical Transactions of 1841. As unfortunately often happens, little notice was taken of it at the time by other observers. In 1852, however, Virchow wrote an article upon it in his Archives, and finally it was brought prominently into notice by the papers of Zenker in 1861 and 1864. Since then numerous observers have contributed to our knowledge of this condition, and among them may be mentioned the well-known names of Waldeyer, O. Weber, Cohnheim, and Neumann.

A portion of muscular tissue which has undergone this "degeneration" presents very marked alterations in its microscopic appearances. The muscular fibres as a rule become markedly swollen and enlarged, they completely lose their fine transverse striæ, and altogether they look like homogeneous, transparent cylinders of glass. These glassy cylinders are marked at intervals with transverse folds, and show a decided tendency to break across into short pieces, the whole cylinder sometimes breaking up into rounded or flake-like particles—hence Neumann's proposed term, "the flaky splitting of muscle."

Chemically, the constitution of the affected part differs little if at all from that of normal muscle.

Zenker showed that this affection is exceedingly common in the muscles of those who have died of typhoid fever, and that it occurs with especial frequency in the adductor muscles of the thigh, the straight muscles of the abdomen, the diaphragm, and more rarely in the great pectoral muscles. To the surgeon it may be of interest to know that its presence in the substance of the rectus abdominis has been connected with the occurrence of spontaneous extravasation of blood and the subsequent formation of dense cicatricial tissue which might easily be mistaken for a tumour.[1]

By other observers the glassy degeneration of muscle has been found to be a common condition in tetanus, acute miliary tubercle, uræmia, smallpox, scarlatina, and a variety of other febrile conditions. It has also been observed as a local result of wounds and bruises. Cohnheim found that he could produce it artificially in the frog's tongue by ligaturing the arteries of supply. Bennett and Billroth have found it in the neighbourhood of tumours. Erb, who thinks it a mere post-mortem change, observed it in the limbs of rabbits, when he had, some time before the death of the animal, divided the crural nerves.

By different authors the change has been classed as a true degeneration; as a mere physical change dependent upon tearing of the muscular fibres and great retraction of the torn ends; and, lastly, as a purely post-mortem condition.

The present author, who pursued his investigations in the laboratory of Professor G. Arnold of Heidelberg, and under his direction, has chosen the tongue of the frog, which is rich in muscular fibre, as the subject of experiment.

His results show very clearly that all the appearances characteristic of this "degeneration" can be produced artificially by injuring the tongue in various ways.

Thus, in experiment 1, the tongue was fastened with pins in the usual way, and the frog, not being paralyzed with curare, struggled violently to be free. The injury thus inflicted upon the tongue very soon produced the "glassy degeneration" throughout all the muscular fibres. In experiment 2, the frog was curarized, and therefore did not struggle. No change was noticed in the muscular fibres till the tongue was wounded by an incision, and then the fibres in the neighbourhood of the wound exhibited the change. In experiment 3, the same result was obtained by dividing some muscular fibres subcutaneously; in experiment 4, by pinching the tongue with forceps. So with other means of irritation, such as the application of hot water to the mucous membrane. Lastly, in accordance with the observations of Cohnheim, it was found that ligature of the vessels of supply likewise produced it.

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[1] Uhle and Wagner's Handbuch, S. 425.

The author, in view of the great rapidity with which he has been able to produce the change, cannot regard it as in any proper sense a degeneration. He finds that dilute acetic acid softens the glassy material in the same way as it does the natural muscular fibre—his experience in this particular differing from that of Zenker. He believes that the change really consists, as was previously asserted by Hoffman and Martini, *in a very rapid coagulation of the myosin or muscular fibrin, with subsequent contraction and splitting up of the coagulated contents of the sarcolemma.*

Amyloid Degeneration of the Kidneys. By Dr LECORCHÉ (Archives Générales de Médecine, September 1874).

THE writer exhibits an intimate knowledge of the literature of his subject. He refers frequently to Grainger-Stewart's work on Bright's Diseases of the Kidney, but occasionally misinterprets it. Although preserving the name "waxy degeneration," he only does so on account of the universality of its adoption, but considers that it is founded on a misconception of the pathology of the disease. Under anatomical characters he takes exception to Grainger Stewart's division into three stages, based chiefly on microscopical examination, and founds upon the naked eye appearances of the organ. He admits only two stages, viz., 1st, The stage of hypertrophy; 2d, The stage of atrophy. His first includes the first two of Grainger Stewart's divisions, viz., 1st, The stage of vascular degeneration; 2d, The stage of transudation into the tubules (which, by the way, Lecorché translates—the stage of tubular degeneration, "période de dégénérescence canaliculaire"). To found upon microscopic characters when one can so well define the microscopical as Dr Lecorché does, is surely a retrograde movement. His description of the progress of the degeneration as evidenced by microscopical examination attests the wisdom of the division into three stages, to which he objects; for he shows that the degeneration of the vessels precedes by a considerable period of time the affection of the other elements, and that it is only when the latter become involved that the organ becomes hypertrophied. He misinterprets Grainger Stewart when he makes him say that the affection at its commencement is invariably localized in the Malpighian tufts, neither does the latter attribute the atrophy of the third stage to changes consequent on a "hyperplasia (interstitial nephritis) of the connective tissue."

Dr Lecorché thinks that he has found evidence that albuminuria never is a symptom of uncomplicated cases of amyloid degeneration of the kidneys, and that when present in this disease it is due to complications with parenchymatous and interstitial (!) nephritis. The grounds for this conclusion he does not give, unless it be the statement in a succeeding paragraph, viz., "With the appearance of albumen in the urine there coincides a diminution of the polyuria, which may even cease altogether when the albuminuria is considerable." But he does not offer to explain the albuminuria in those cases, which are not uncommon, in which there is no diminution of the polyuria.

Dr Lecorché also considers that interstitial nephritis is not less common a complication than parenchymatous. Such is not the experience of all; neither does that affection occur, as an independent disease, only in old people (as is stated in an early part of the paper); it is also met with, although much less commonly, in younger persons.

In considering the pathological character, Dr Lecorché sides with those who hold it to be, not a degeneration of the normal tissues of the organ, but an infiltration of these by a morbid material. To convince oneself of this he says that it is only necessary to submit these tissues to microscopical examination; "one then sees that in the midst of an amorphous substance, which is nothing else than the amyloid matter, he can distinguish the elements proper of the kidney (tubules, glomeruli, arteries)." Those who believe it to be a degeneration acknowledge that in the later stages there is an infiltration (the infiltrated material taking no mahogany colour), but they interpret the appearances seen in the earlier stage as proof of degeneration. One can often see, at this time, in one of the larger of the affected vessels, the muscular fibres of the middle coat lying in no amorphous material, but themselves the subject of change, which causes them to take the deep mahogany coloration with iodine, which makes them such prominent objects in the field of the microscope.

Croup in its relation to Tracheotomy. By J. SOLIS COHEN, M.D. Philadelphia: Lindsay and Blakiston: 1874. Pp. 78.

THIS volume was read as an essay before the Philadelphia Medical Society in January 1874, referred by that Society to the Medical Society of the State of Pennsylvania, and by the latter Society ordered to be printed in their Transactions for the current year. It is based on a careful study of more than 5000 cases. The author starts by defining croup as an exudative inflammation of the air-passages, and deals with it under two heads: 1. Sthenic croup, or croup proper; and, 2. Systemic croup, or the croup of diphtheria. He finds that in his native city and state tracheotomy for croup is not approved, is not often performed, and is seldom successful. The object of the work seems to be to show the advantages of the operation in suitable cases, and to give rules for its performance.

That the extreme want of success which attended the first efforts of some operators should be no bar to perseverance, is brought out very forcibly by some striking statistics derived from the experience of Trousseau, Barthez, and others. The early want of success of some is accounted for by the fact that some surgeons seem to think that their duty was over once they opened the trachea.

Tracheotomy does not cure croup; it only gives a chance to the powers of nature to recover themselves, and enables the patient to live and fight through the development and decline of the disease.

To two London surgeons, Andrée and Chevalier, the honour is given of being the first to operate for croup—one in 1782, the other in 1814. Bretonneau, Stolz, and Trousseau followed on the Continent, each, and especially the latter, with a considerable experience. Some of the statistics are very remarkable, especially in the extraordinary fatality, and yet extraordinary perseverance of some of the early operators. Thus, previous to 1849, the operation had been performed 49 times in the Hospital for Sick Children in Paris, with one recovery; while in the next ten years, of 991 operations, 233 recovered. Many of the best cases, and about the best percentage of recoveries, are obtained in the pages of this Journal, especially the remarkable statistics of Mr Spence in Journals for February 1860 and March 1864, in which he has 28 recoveries out of 87 cases.

Age influences success very much; few—only 33—cases operated on under two years of age have survived. The youngest successful case on record, if we except that of Scoutetten of Strasburg, in which the disease was doubtful, is still that of Mr Joseph Bell, of Edinburgh, reported in this Journal for 1861, p. 956, at the age of six months.

The indications for the operation are carefully discussed; the question of administration of chloroform or not is decided by a strong opinion in its favour from many competent observers; and we are glad to see that the mode of operation by slow, cautious dissection is recommended in preference to any more rapid method by puncture or bold incisions. M. Trousseau's famous speech is quoted: "A certain surgeon has reproached me with operating like a physician and not like a surgeon. Well, he had an opportunity on one occasion to operate like a surgeon, and with one stroke of his knife he divided the œsophagus as well as the trachea. Some time afterwards he lost another patient by hæmorrhage during his brilliant operation. Since then he has operated more like a physician, and after a while he will become a very fair tracheotomist." P. 38.

The following are the conclusions drawn by the author of this laborious and well-reasoned work:—

"1. That there are no insuperable contraindications to tracheotomy in croup.

"2. That the administration of an anæsthetic for the purpose of controlling the child's movements is admissible in performing the operation, but that it should be used with great caution.

"3. That a careful dissection should be made down to the windpipe, and hæmorrhage be arrested before incising it, whenever there is at all time to do so.

"4. That the incision should be made into the trachea as near the cricoid cartilage as possible to avoid excessive hæmorrhage and subsequent accidents, which might cause emphysema.

"5. That a dilator should be used, or a piece of the trachea be excised, whenever any difficulty is encountered in introducing the tube.

"6. That the tube should be dispensed with as soon as possible, or altogether, if the case will admit of it.

"7. That assiduous attention should be bestowed on the after-treatment, especially that of the wound, and that a skilled attendant should be within a moment's call for the first twenty-four or forty-eight hours immediately following the operation." P. 78.

Remarks on the Uses of Bazaar Medicines, and Common Medical Plants of India. By EDWARD JOHN WARING, M.D. Second Edition. London: J. and A. Churchill: 1874.

THIS is a very interesting little volume, and is calculated to be of immense value to those for whose benefit it was specially written. The author is the able editor of the *Pharmacopœia of India*, and is well qualified to write a popular work on bazaar medicines and common medicinal plants of India. Dr Waring has produced a book full of useful and interesting information, which, in the hands of judicious men, will prove of great benefit to thousands who are beyond the reach of properly qualified medical practitioners.

The object of the author is a humane and noble one, namely, "to point out the agents, either purchasable in the bazaars at an almost nominal price, or procurable at the cost of collection from the roadsides, waste places, or gardens, in the immediate neighbourhood of almost every out-station,"—by means of which, with the exercise of ordinary intelligence, educated men may be enabled to relieve the sufferings of those amongst whom their lot has been cast.

The author tells us that his book is especially designed for "missionaries, whose stations for the most part are too far removed from the busy haunts of men to allow of their calling in regular medical aid in cases of sickness, and who are constantly thrown at such times very much on their own resources;" for "European and Anglo-Indian officials at distant up-country stations, miles away from medical aid;" for "educated natives, who readily, as a class, avail themselves of every scrap of knowledge, drawn from trustworthy European sources, which tends to throw light on the products and resources of their native land;" and, lastly, "for duly qualified medical officers."

The author has made a most careful selection of the best medicines procurable at Indian bazaars, having due regard to the safety of the drug, its acknowledged utility, its possessing a recognised native name, and by its wide distribution and easy procurability in all parts of India generally. The description of the plants and other medicines is such, that mistake is almost impossible when due care is observed. When a poisonous drug, such as opium, is introduced, Dr Waring gives such valuable directions regarding its administration, that little danger is apprehended. The book also contains a good "index of diseases," which will prove of great benefit to those for whom the work was written. Altogether, this is a highly useful and well-written work. We wish it a wide circulation in India, and hope the earnest wish of the author will be realized, that it will "constitute, as far as India is concerned, a missionaries' medical vade mecum."

A Universal Formulary: containing the Methods of Preparing and Administering Officinal and other Medicines. By R. E. GRIFFITH, M.D. Third Edition, by JOHN M. MAISCH, Phar. D., Professor of Materia Medica and Botany in the Philadelphia College of Pharmacy. Philadelphia: Henry C. Lea: 1874.

THIS work stands in much the same relation to the Pharmacopœia of the United States that "Squire's Companion" does to that of Great Britain. "The design of the work is to present a compendious collection of formulæ and pharmaceutical processes, with such additional information as may render it useful to the physician and apothecary." The introduction contains tables and observations on the weights and measures employed for pharmaceutical processes in the United States and in foreign countries, and a vocabulary of the principal abbreviations and Latin terms used in writing prescriptions, followed by observations on the management of the sick-room, with rules for the administration of medicine.

The great body of the work is occupied with the "Formulary," in which is given the method of compounding the various officinal preparations of the United States, together with the chief preparations of the pharmacopœias of Britain and other countries. It also contains the composition of many favourite mixtures of the leading physicians of the United States, many of which are more curious than important. The work does great credit to its author and editor, and will prove an excellent *work of reference* to physicians and druggists, especially to those of the United States.

Statement of the Grounds on which the Contagious Diseases Acts are opposed. By J. BIRKBECK NEVINS, M.D. Lond. Addressed to the Right Hon. R. A. CROSS, M.P.

NO one nowadays considers it advisable to come to a conclusion on any subject on the faith of statistics alone. This holds with especial force with regard to the subject of which the present pamphlet treats.

Both opposers and advocates of the Contagious Diseases Acts bring forward what appears to the ordinary observer incontestable proof of the right of their own special side. The process by which this happy result is arrived at, is termed "cooking statistics."

One point, however, on which both sides are equally agreed, is the *reduction in number of public prostitutes* in the towns subject to the Acts. This fact, admitted alike by advocates and opposers, is to our mind the strongest possible argument against the Acts. What results in reducing the number of public prostitutes by repressive legislation? This is the result: the widespread distribution of clandestine prostitution, with all its attendant evils, among the general population.

The history of repressive legislation applied to prostitution has always been the same, since the time when, in the sixteenth century, prostitutes were branded on the cheek with a red-hot iron, until the present day, when we doubt not the Contagious Diseases Acts will prove as abortive as the more cruel enactments of our forefathers.

Dr Nevins's Statement is a careful and comprehensive compilation of most that can be said against these Acts.

Outlines of Zoology and Comparative Anatomy. By MONTGOMERY ALBERT WARD, M.B., M.Ch. Univer. Dub. Dublin: Fannin and Co.: 1874.

IN this work, extending to 150 pages—inclusive of title, preface, contents, an appendix containing a list of human parasites, and examples of examination questions—the author endeavours to lay before his readers the "leading features of zoology and comparative anatomy." He wisely in his preface tells us, that he lays "claim to no originality," and we cannot give him credit for making, even the more salient points of the now extensive science of zoology, plain or intelligible to "persons who may desire to obtain only a general knowledge of these subjects"—to quote again the words of the prefatory notice. It appears to us that the popular reader and the elementary student, more than the advanced pupil, should surely derive more benefit from the consultation of works in which the science is at least fully treated. In such a small volume as that before us, it is manifestly impossible to expect that explanatory detail, so essential to the successful study by the junior student, of even the simplest biological phenomena. Hence Mr Ward's labour is of no account in either the teaching or advance of zoological science. When we mention that the sub-kingdom Annuloida is dismissed with three pages of treatment, the Cœlenterate animals with barely two pages, and the Protozoa with three and a half pages, it will be evident that the junior student's mind will require to be of singularly appreciative and apt character, if he is to benefit from a perusal of Mr Ward's work. Nor can we see how the question—to select one of many given in the appendix—"Give an idea of the structure of a sea-urchin," could be answered at all by a student who had read Mr Ward's manual only. In the section on the Annuloida, the "idea" given of a sea-urchin practically amounts to no idea whatever. Altogether, we cannot allow that any demand existed for Mr Ward's book, seeing that we already possess far more comprehensive and well-illustrated manuals. There is only a solitary woodcut, illustrative of the "ideal vertebra" of Owen, in the entire book. We have seen a diligent student's note-book which comprised far more information than that given in the work before us.

Botanical Tables for the use of Students. Compiled by EDWARD B. AVELING, B.Sc. London: Hamilton, Adams, and Co.

A SERIES of tables which may be useful to a certain class of botanical students. Nevertheless, from the fact that all modern text-books contain similar tables, we cannot see that there was any special call for the present series being issued. Should Mr Aveling contemplate bringing out a new edition, we would suggest that he should add a table of the Cryptogamia treated in the same manner as the Phanerogamia. We don't quite agree with his classification of fruits. Surely it is an error to describe the fruit of *Typha* as a "drupe."

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING III.

Wednesday, 6th Jan. 1875.—Dr D. R. HALDANE, *President, in the Chair.*

I. *Mr Annandale* showed—(1.) A little BOY upon whom he had operated for traumatic arterio-venous aneurism of the popliteal artery and vein. The interest of the case was, that he had ligatured both the popliteal artery and vein with the most satisfactory result, a proceeding which had, as far as he was aware, never before been adopted in this disease.

(2.) A MAN whose entire tongue he had removed for epithelial cancer. The patient had made an excellent recovery, and as it was the first case of recovery after removal of the *entire* tongue in the Edinburgh Infirmary, he attributed the good result in some measure to the particular method in which he had performed the operation. His method was, to expose the entire tongue by dividing the lower jaw at the symphysis, and then to split the organ and remove each half separately by means of the ecraseur.

(3.) A PATIENT who had sustained a fracture through both rami of the lower jaw. As the fragments could not be kept in position by any ordinary means, he had made an external incision over the fracture on both sides, drilled the fragments, and brought them together by means of silver wire. The result had been completely successful, and the patient could now masticate and use both jaws freely.

(4.) A large GOITRE which he had removed from a patient who was suffering from difficulty of breathing, owing to the pressure of the tumour. The operation was performed by thoroughly exposing the tumour and tying the bloodvessels where they entered it before dividing them. The gland was firmly adherent to the trachea for fully two inches; but, by careful separation with the finger-nail, this attachment was successfully severed, and the entire organ removed. There was a little troublesome bleeding from a lower thyroid vein during the operation, but the patient bore the operation well, and had a good pulse after it was finished. An attack of hæmorrhage occurred the night of the operation, and the patient died in consequence. He showed it to confirm what had already been proved by Dr P. H. Watson, that even a large goitre could be excised.

(5.) A preparation illustrating OSSEOUS ANCHYLOSIS OF THE KNEE AND ANKLE JOINTS, which he had obtained from a patient who had long suffered from chronic ostitis of the tibia, involving these two joints. He had amputated the limb to relieve great pain, which had resisted all ordinary treatment, including trephining of the tibia.

II. *Mr Joseph Bell* made a verbal communication on the subject of the very successful treatment of LEPROSY, by Dr Dougall, among the convicts at the Haddo Leper Hospital in the Andaman Islands. See previous articles in this Journal for July 1874 and January 1875. *Mr Bell* showed specimens (1) of the balsam in its pure state; (2) of the emulsion as prepared for internal administration; (3) of the ointment as used by Dr Dougall externally. He described the wonderful improvement that had been gained in the condition of every one of the poor lepers in the place, and gave the following brief account of the drug and its administration:—

Gurjun Oil or Balsam.—According to Sir R. Christison this is not an oil, though usually called so, but a *turpentine*, with little resin, much volatile oil, and nothing else, not a little resembling copaiva, for which it has been used for a good many years in India for gonorrhœa internally, and outwardly for rheumatism. About twenty years ago it was introduced into England as a substitute for copaiva balsam, but did not succeed in securing a place in the Pharmacopœia. Dr Fayrer had found it useful in dysentery, in genito-urinary affections, and in healing up old ulcers, but now heard of its use for the first time in treating leprosy. This oleo-resinous substance is obtained in abundance in the Andaman Islands, and on the Malayan coast of the Bay of Bengal, from the *Dipterocarpus lævis*, and other trees of allied genera. It cannot be conveniently used without a *solvent*, such as cocoa-nut oil, castor oil, or lime water.

Gurjun Ointment consists of lime water three parts, and gurjun oil one part. Mix and agitate violently till they unite thoroughly and form a substance of the consistence of Indian butter. It runs as smoothly as the ordinary soft soap, and can be applied without the slightest disagreeable consequences; no pain follows its use; no blistering when rubbed on the healthy skin, and the ointment is not malodorous. It keeps well, especially when not too freely exposed to the atmosphere in bulk. At Port Blair it is kept in a large jar with wooden lid. A very simple machine will prepare twelve pounds of ointment in five minutes.

Gurjun Emulsion consists of equal parts of lime water and gurjun oil. It is not disagreeable to the palate, and the lepers took half-ounce doses night and morning, and constantly asked for more. At first it had in small doses no well-marked influence on the digestive system, but when the dose was increased to one drachm twice a day, it improved the appetite and acted as a mild laxative. It has also a distinct diuretic effect. The larger dose (4 drachms) twice a day caused several large healthy motions—in fact, acted as a powerful diuretic and evacuant.

Professor Erasmus Wilson has used a liniment of equal parts of gurjun oil and lime water in cases of painful eczema, in lupus, and in cancer, with very encouraging results, and he stated Mr Hancock had applied it in a case of cancer of the skin with the effect of dispersing tubercles and healing ulceration, but its most useful property was that of relieving pain.

The supply of the oil is inexhaustible, and it costs the Government less than 8 annas, or 1s., per gallon for collecting it; and as an indication of the increasing demand for it, for the treatment of leprosy, we learn by last accounts that the following quantities were forwarded fron Port Blair within three months, viz., 2000 lbs. to Madras, 2000 lbs. to Bombay, and 3600 lbs. to Calcutta; part of the latter being for the Mauritius Government.

III. *Dr M'Kendrick* then showed a set of ELECTRO-PHYSIOLOGICAL APPARATUS, made for him by Mr Hart, instrument-maker, Edinburgh. It included (1) various kinds of clamp forceps; (2) polarizable electrodes; (3) non-polarizable electrodes; (4) various kinds of keys; (5) an electromotor; (6) tetanometer of Heidenhain; (7) trip hammer of Pflüger; (8) commutators; and (9) a new kind of electrode for experiments on the rapidity of the nerve current.

IV. *Dr Cadell* then showed a WAX CAST OF A PENIS, with an uncommon form of primary syphilitic lesion. It consisted of induration only, without any ulceration of the skin. There was slight desquamation of the cuticle, from the most prominent part of the induration. The resulting secondary symptoms were mild.

V. *Mr Chiene* then read a short paper, entitled BLOODLESS SURGERY, which will appear in a future Number of this Journal.

The President thought that Mr Chiene had in his paper fairly established his theoretical objections to Esmarch's method, and proved that the methods already in use in the Infirmary were superior.

Mr Joseph Bell remarked that he was glad to hear Mr Chiene's able defence of good old conservative surgery. From his own experience he could thoroughly support most of the objections to Esmarch's method, brought forward in the paper. He had now employed it in many operations which he had performed in the country, because he had found that the practitioners under whose care the patients were, expressed anxiety to see it used, and were much interested in it. In all these the plan succeeded admirably in arresting all hæmorrhage, until the tying of the vessels. In one special case of amputation of the thigh, a white handkerchief placed below was perfectly unstained during the cutting, but there was a considerable amount of jetting from the small arteries when Esmarch's tourniquet was removed; so much so, that unless Petit's tourniquet had been also in reserve above, there might have been more blood lost than was desirable. His general impression was, therefore, that while the bloodless method might be easily managed, and prove successful enough in hospital practice, where there are numerous and skilled assistants, it was not one so suitable in private operations, where assistants are few. He, however, held that in certain instances it was exceedingly useful, some of these being the extraction of a needle or other foreign body from the finger or any part of a limb. Here, by means of an ordinary umbrella indiarubber ring, the finger could be rendered perfectly waxen, and the needle sought for without being in any way obscured by blood. A further advantage was, that the pressure of the ring acted as an anæsthetic. As to the origin of the plan of lessening bleeding by previously elevating the limb, he had always thought that it was one of the traditions of the Edinburgh Infirmary. He also mentioned that in a recent American journal he had noticed that this subject of "bloodless surgery" had been engaging the attention of the surgeons of that country. The conclusion they seemed to have arrived at was, that it was better to use a *split* indiarubber tube as a tourniquet instead of Esmarch's one, because in one instance the pressure of Esmarch's tourniquet had caused a circular slough of skin.

Mr Annandale said that he had seen Esmarch's bloodless method employed, but that he never used it himself, as he was of opinion that Mr Lister's method was perfectly satisfactory.

Mr Black mentioned that Dr Keith of Aberdeen was in the habit for many years of bandaging and elevating the limb previous to amputation, especially in the case of elderly people, in which it was important to save blood. He had also seen Esmarch's method used lately in St George's Hospital in London, but there they used an ordinary tourniquet instead of the elastic cord.

VI. *Dr Coghill* then read an account of a case of HYDRONEPHROSIS affecting the right kidney. The patient, an unmarried lady, aged 30, consulted him on account of a swelling occupying the right lumbar region. There was a history of painful micturition for many years, which, during the last four, had become protracted and paroxysmal, one of these paroxysms being coincident with her menstrual period. The pain was always severe, and, starting from the right lumbar region, passed downwards to the limb of the same side. Her condition was much aggravated by a faulty regimen—including a large quantity of animal food and malt liquors. To obtain relief many remedies had been used, including, among others, chloral and tonics. These, however, did not mend matters. Bicarbonate of potash, taken on the advice of a non-professional friend, brought on an attack of hæmaturia, during which the tumour, from patient's straining apparently, was dislocated downwards and forwards. Examination of the urine showed that it possessed a high specific gravity, and excess of acid salts; but no albumen nor pus. After keeping the patient under his observation for three weeks, Dr Coghill determined to tap the tumour, and therefore passed an exploring needle upwards and forwards into it. Its capsule was then found to be dense and elastic, so that the fluid jetted out with some force, and therefore Dieulafoy's aspirateur, which had been provided, was not required. In this way, four pints, six and a half ounces of a straw-coloured fluid were drawn off. On examination, the fluid was found to have a sp. gr. of 1004·5. No albumen nor pus could be detected, but crystals of cholesterine were found; in fact, its composition was nearly allied to that of the urine. Dr Coghill next alluded to the pathology of this case. Most probably a renal calculus had become impacted in the right ureter, and thus obstructed the flow of the urine on that side. From the pressure due to its accumulation, there had been gradual dilatation of the pelvis and calyces, and ultimately atrophy of the kidney tissue. In conclusion, he noticed the fact of its comparative rarity in the adult, and greater frequency as a congenital defect; and referred to the literature of the whole subject. He also drew attention to the dislocation of the tumour, and the composition of the contained fluid. Especially as to the latter point, he would feel obliged by any of the members giving him accounts of, or reference to, similar cases, as he had been unable to find a case identical in character on record, where the nature of the tumour had been detected during life, and operated on with success.

The President expressed his interest in Dr Coghill's case. He felt sorry that he could not at the present moment give exact references; but he distinctly remembered that when Pathologist to the Edinburgh Infirmary, he was requested by Professor Spence to examine some fluid removed from a cyst the nature of which was obscure. This fluid was almost identical in physical characters with that drawn from Dr Coghill's patient, but it contained no cholesterine, and the presence of urea was readily recognised.

VII. *Dr Craig* then read—

NOTE ON JABORANDI.

In the Periscope of the *Edinburgh Medical Journal* for July last, I gave an account of the remarkable medicine called jaborandi. After furnishing the note, I tried to procure some of this wonderful plant, with the view of testing its alleged virtues. It was only yesterday that I was fortunate enough to procure a supply. When I made application for it during last summer, it was not to be had in this country, and the small quantity sent to Paris had all been consumed by experimenters in that city. Through a friend in London I was fortunate enough to secure one drachm of this drug, being all that he could procure for me, and even that small quantity was given him as a special favour. As the dose of jaborandi is said to be from sixty grains to eighty grains, it will be perceived that I had only the minimum dose with which to perform an experiment. The little, however, which I possessed, I carefully infused with a teacupful of boiling water for two hours. The infusion was then allowed to become cold, after which, the whole cupful of infusion was swallowed, and the results carefully observed. Notwithstanding its peculiar odour, it was not unpleasant to the taste, and produced no nausea. About twenty minutes after the infusion was swallowed salivation commenced, and in a few minutes afterwards the mouth was literally flowing with water, smelling strongly of jaborandi. Simultaneously with the flow of saliva the perspiration appeared on the forehead and over the whole body. It extended to the limbs, but was most marked on the forehead and trunk. This continued for fully four hours. The temperature of the room during the time of the experiment varied from 54° to 56° F. The temperature of the patient was unaffected by the jaborandi, having remained normal throughout the whole experiment, confirming what had been noted by other observers, that the effects of jaborandi were in no way depending on the rise of temperature of the body. It appears to act by directly stimulating the sweat-secreting glands.

It is very much to be regretted that I had only sufficient to perform one experiment, and that, too, the minimum dose of the drug. Nevertheless, the members of the Society will perceive that even from that one trial it is clearly proved that the sialagogue properties of the plant are well marked. In less than three hours I collected fully ten ounces of clear fluid from the mouth. Certainly I know of no medicine, either in the Pharmacopœia or out of it, which possesses sialagogue properties at all to be compared to jaborandi. From my small experience, I am very much of the opinion of Professor Gubler of Paris, that it is an "incomparable sialagogue." Although I found it a powerful diaphoretic, still the amount of perspiration was not so great as I had anticipated from reading the accounts of the experiments at Paris. This much, however, is to be borne in mind, that I had only the minimum dose of the drug, and it is also quite possible that the substance was not fully exhausted in the infusion which I prepared, nevertheless its effects as a diaphoretic were very well marked. The leaves were reduced to a coarse powder when I received them, and so I had no means afforded me of determining the species to which jaborandi belongs. Some assert that it is a plant belonging to the Rutaceæ, others that it belongs to the Piperaceæ; whether or not it belongs to either of these natural orders, it is impossible at present for me to say. I think it right to mention, that in a dictionary in Portuguese of the medicinal plants of Brazil, the jaborandi is spoken of as *Ottonia anisum*, a plant belonging to the Piperaceæ. This work was published in 1868 by Dr Chernoviz. He mentions that the root of jaborandi has sialagogue properties, and that from the root a tincture is made (one of the root to eight of alcohol), which tincture is used as a sialagogue. It will be interesting to learn whether or not the jaborandi of Dr Chernoviz is identical with the jaborandi at present under consideration. I need scarcely say that I am most anxious to procure more of this wonderful drug, with the view of fully investigating its physiological and therapeutic qualities; and if I am fortunate enough to get more of the plant, I hope on some future occasion to be able to present to the Society a communication worthy alike of the Society and of the interesting drug called jaborandi.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIV.—MEETING II.

Wednesday, 9th December 1874.—Dr Matthews Duncan, *President, in the Chair.*

I. FOREIGN BODIES IN THE FEMALE BLADDER. BY JAMES YOUNG, M.D.

Among the products of certain pathological conditions in relation to the kidney or bladder might be enumerated hair masses simulating calculus, hydatids passing from the kidney, worms which are regarded of doubtful existence, tumours or polypi, and different kinds of calculi. Cases have occurred where instruments have been broken into the bladder, or become lodged there in one way or another, causing grave disturbance. In the case which I will now briefly mention, a foreign body was lodged in the bladder by accident. Miss A. B., aged 27, suffered for some months from irritable bladder, with dysuria. She had been under my care for several months previous in consequence of an hysterical condition, accompanied with great wakefulness. I never prescribed for the vesical irritation. Two months ago this patient suffered great pain and difficulty in passing water, and, accordingly, actuated by her own idea to aid the extrusion of the urine, she passed along the urethra a bone penholder, six inches long, with the view of dilating the passage, but the instrument slipped from her fingers and lodged itself in the bladder. I saw her next morning, and again at four P.M., when the bladder was full. On examination with the sound, I felt it impinge on the foreign body. I returned at ten P.M., and used a pair of ordinary dressing forceps, passed into the bladder, while the left index finger was in the vagina. I easily felt and seized the penholder (which was described to me by the patient). The foreign body was always grasped about the middle, which prevented its removal. Guided by the left index finger through the vesico-vaginal wall, I endeavoured to slip the forceps to the end of the holder, and having succeeded in doing so, I got the lower end by the left finger tilted forwards to the meatus urinarius, and easily extracted it. No untoward symptoms supervened from the manipulations, beyond some degree of pain for a day or two. The patient refused to take chloroform.

Dr Duncan mentioned a somewhat similar case, which was told by Mr Joseph Bell at a recent meeting of the Medico-Chirurgical Society, in which part of a parasol-handle had been pushed into the bladder of a young female, and was extracted after dilatation of the urethra.

Dr Simpson could not understand the penholder slipping in, unless it had been pushed in by the patient. He had not met with a similar case in a female, but in a man dying of opium-poisoning to whom he was called by another practitioner, an old gum-elastic catheter had been passed by the gentleman who had called him in, and, on withdrawing it, fully half was left behind in the urethra, where it could be distinctly felt. A few minutes later it was sucked into the bladder—a similar thing may have happened in Dr Young's case.

Dr Young explained that his patient was an exceedingly nervous person, and in her excitement she had let the penholder slip.

II. A paper by *Dr Imlach* on RETROVERSION AND PROCIDENTIA AS ALTERNATING MISPLACEMENTS OF THE GRAVID UTERUS, which was communicated by the President, will appear in a future number.

Dr Simpson thought that all of the Fellows must have seen cases of retroversion of the gravid uterus and of procidentia, but he had not met with a case in which retroversion occurred after replacement of a procident gravid uterus. Retroversion was common in cases of prolapsus in the non-pregnant state, and in that condition he could easily believe in a connexion between the two, and he thought Dr Imlach's explanation of the mechanism of the displacements was correct. The recurrence of retroversion in successive pregnancies was not uncommon. He had a case under his care at the present time. He had never failed to reduce them by manipulation, and would not feel justified in leaving a distended bag in the rectum side by side with a pregnant uterus, considering such practice liable to do mischief. He thought the pressure should be intermitted at short intervals.

Dr Gordon had met with two cases of retroverted gravid uterus, in one of which it had recurred;—on each occasion it was easily replaced.

Dr Coghill thought that retroversion of the gravid uterus was commonly caused by overdistension of the bladder. He had one case in which he could not effect reduction until he had put a finger into the cervix and pulled it down, thus producing procidentia to a certain extent.

Dr Keiller had seen and reported to the Society several cases of retroversion of the gravid uterus. It was in a case of this kind that he was first led to *stethoscopize the uterus per vaginam*, with the view of determining the existence of pregnancy: a mode of investigation which he had often since then found of great use in many doubtful cases. In one case the patient came to see him, suffering from violent bearing-down pains. He found the gravid uterus retroverted, and by placing a stethoscope on the fundus in the vagina he distinctly heard the uterine souffle; reduction was effected, and the patient did well. In this case there was no distension of the bladder. In another case which he had seen with Dr Bruce, he had heard the uterine souffle in a similar way. Reduction was difficult, but was at length effected by placing the fingers of left hand behind the fundus, while the thumb was inserted into the cervix, and so acting on the whole organ at once. The connexion between procidentia and retroversion may be as specially suggested in the paper now read, but he could not agree with the remark of Dr Imlach, that rectocele is not at all met with in cases of procidentia.

Dr Duncan quite agreed with the theoretical part of the paper, but he thought the connexion between procidentia and retroversion, both of the pregnant and non-pregnant uterus, was even more close than Dr Imlach stated. Among other cases of retroversion of gravid uterus, he had lately seen a lady who had the general symptoms of retroversion without any retention of urine. He could not replace the uterus, but as the pregnancy was yet in the early months he sent the patient home, and told her to lie on her knees and face for half an hour at a time, several times a day. The retroversion soon got right; but it recurred, and was attended by prolapsus; the same treatment produced a satisfactory result, and the patient was successfully confined at the full time. He had once replaced a retroverted gravid uterus in his own house, as the patient had come from a distance and was anxious to return home; but he did not consider this a desirable proceeding. The most remarkable case he had seen had been already published. All the pelvic organs were matted together into a mass. The patient again became pregnant, the uterus being retroverted, and growing down into the pelvis, till the fundus was near the perineum. She went to near the full time, when the fœtus died. As no labour came on at the full time, he insinuated his hand into the os with difficulty, and pulled out the child. The patient recovered, and is still living. He had delivered in cases of placenta prævia in the same way, and had found the uterus to contract perfectly well after it.

III. The following paper on a FATAL CASE OF POST-PARTUM HÆMORRHAGE, by *John Connel*, M.D., Peebles, was read:—Isabella R., a mill-girl, aged 20, primipara, was confined of an illegitimate female child on the 24th of April last. She had been leading a loose life for a considerable time, had been unable to work, and had been taking little or no food for a month previous to her confinement. Her mother died five years ago at the age of 33, of incontrollable hæmatemesis, with splenic enlargement.

I was first called to her at three A.M., and finding the os uteri but little dilated did not remain with her, or return till sent for again about eight. The head was then approaching the perineum, the membranes having ruptured a short time previously. Labour went on satisfactorily till during the last half-hour, when, the head being unduly detained, I completed delivery with the short forceps at half-past nine.

The uterus was fairly contracted, and for a short time all appeared to be going well. It was noticeable, however, that at no time did she show signs of the pleasurable reaction commonly seen just after the birth of the child. Keeping my left hand on the uterus, I allowed a few minutes to elapse before passing my finger along the cord to feel the condition of the placenta.

The margin was presenting through the os, and I could feel a considerable flow of blood escaping by the side of it. Before I could introduce my hand further she suddenly exclaimed "Oh, what's this? I'm a gone woman!" and at the same time the placenta escaped, accompanied by a large quantity of florid blood, which was projected to near the foot of the bed. I at once used all possible means to arrest the hæmorrhage, but though there was fair contraction of the uterus it still continued. Seeing the critical condition of the patient I sent for Dr Fergusson, and with his consent (having previously injected cold water without avail) I introduced, about half an hour after delivery, three ounces of liq. ferri perchloridi, diluted with three times its bulk of water. The flow of blood ceased at once, and gave place to a blackish watery fluid, no further uterine contraction occurring.

At this time the pulse at the wrist was imperceptible, and the dependent parts of the body, chiefly the left arm and hand, and the left side of the head and neck, were livid.

I thought she was dead, but was comforted by the remembrance of what I had read of Dr Barnes's success with the perchloride. After a short interval she rallied so far as to be able to swallow fluid food and brandy, which were freely administered by mouth and rectum. The pulse returned, and I began to speak to her confidently of recovery. She kept saying, however, that "she was a gone woman." For an hour and a half she seemed to be doing well, when, about 12.15, she suddenly threw herself round on her hands and knees, and expired within a few seconds.

No post-mortem examination was permitted.

I am aware that this case hardly raises the discussion of Dr Barnes's treatment, as it proves little either for or against it. Knowing, however, that a strong hostility exists in some minds to the use of the iron solution, I trust the members present will give their opinions freely. My own opinion is that the hæmorrhagic diathesis was present in this case, because the uterus was as well contracted as we see it in three-fourths of all the cases that do well. Cessation of bleeding followed immediately on the injection, but the question in my mind was, whether the cold and irritating fluid did not add materially to the shock and collapse induced by the first flooding.

The sudden muscular exertion just before death doubtless hastened the event, if it did not directly cause it. There was no jactitation previously, though some red blood had begun again to issue from the vulva.

Dr Barnes of course admits that there are cases where transfusion may be necessary, but here no time was given for it. Having carefully studied all that had been said as to the success of the iron treatment, I must say my faith got a shock from the result of this case. She probably did not die because of it, but at all events she died in spite of it.

Dr Rattray had lately had a case of very severe post-partum flooding. When he was called to the patient she was livid, pulseless, and speechless. Under the bed was a pool of blood. By removing clots from the uterus, by the employment of pressure externally, and by the diligent use of ergot and brandy internally, the hæmorrhage was checked, and the patient recovered.

Dr Macdonald feared that the discussion would not be a very productive one, as he thought that there were very few practitioners in the Edinburgh School who had made trial of the perchloride of iron. He had only seen one case in which he would have used it, but he did not have it with him at the time; this ended fatally, and, as a *dernier ressort*, he would have felt justified in its use. On another occasion when dealing with severe hæmorrhage post-partum, he sent for the perchloride, but fortunately the bleeding ceased ere he could get it used. He had read the literature of the subject as dispassionately as he could, but was convinced that the balance of results is against the use of the perchloride. The ultimate results of the recorded cases which had been treated with the perchloride were certainly not encouraging; and seeing that the vast majority of bad flooding cases recover under the ordinary means of treatment, he felt driven to the belief that the use of the perchloride introduces new dangers of its own. Again, theoretically, it does not seem calculated to stop the bleeding after labour; the bleeding vessels are too large. It answered very well for serous oozing, and hæmorrhage from small vessels, but it had not power enough to act on large ones. If again we follow Leopold and look upon the mucous membrane of the uterus as a huge lymphatic gland, in close connexion with the general lymphatic circulation, we can easily understand how septic diseases are set up by the putrid clots and the irritation of the mucous membrane caused by the perchloride. The clots found in the venous sinuses likewise could not fail to create great risk of embolism. In making these remarks he was dealing with the subject generally; as regarded Dr Connel's case, he wished it to be clearly understood that he passed no unfavourable criticism. Indeed, under such circumstances he would probably have done the same as Dr Connel did. He would like to know whether ergotine had been tried by the Fellows in such cases; he had used it several times, injecting subcutaneously about four grains, with instant good effects. It had the advantage over ergot administered by the mouth, that it was independent of the aid of the weakened or obliterated assimilating power of the stomach.

Dr Bell was at a loss to know on what principle perchloride of iron is used. Does it produce contraction? If not, it is very injurious, and surely led to embolism. He had always succeeded in checking post-partum hæmorrhage by the ordinary means, and thought that, even if the patient survived the application of the perchloride, inflammation was sure to take place. He strongly objected to the practice.

Dr T. G. Balfour agreed with Dr Macdonald's remarks, except that reports tell us that the perchloride certainly does stop hæmorrhage. He had used ergotin in hæmatemesis in a bad case, and there was no return of the bleeding.

Dr Macdonald explained that he considered perchloride of iron an unsuitable hæmostatic for post-partum cases. No doubt it might stop hæmorrhage, but it probably owed its capability to do so to its power as a stimulant of uterine contraction.

Dr Simpson did not think that, on the evidence hitherto published, the use of perchloride of iron should be set aside; he would be sorry to think that he would not be justified in using it in a suitable case. He had once used it in a case he was called to in consultation, but too late, as the patient was just sinking; but he was glad to think that the perchloride might possibly arrest the flooding, if applied a little sooner. He did not consider the theoretical reasoning against it sound, because the stimulant action would tend to make the uterus contract, and thus the styptic power of the remedy would be applied to a smaller placental surface, and to smaller orifices. Moreover, it is a grand antiseptic; there is a great tendency to septic absorption in these cases, but least of all in cases where the perchloride has been used; hence, though his own experience had not furnished proof of its value, he would like to see further trial made of it in suitable cases. In estimating the worth of the remedy, the difficulty lay in knowing whether the patient whom it seemed to save would have died had it not been used. He thought Dr Connel's case showed that it did arrest hæmorrhage for a time, which was just what Barnes claimed for it. The death was probably caused by the hæmorrhage, slight though it was, which had recurred.

Dr Keiller thought that, without trying the remedy in suitable cases, we ought not to decide either for or against Dr Barnes's mode of treatment. He had not tried it, simply because he had not met with a case which required it. He thought too much was often done in these cases, and that we could generally save our patients if we only set ourselves to husband the little stock of life which was left in them. He had seen many pulseless and apparently dying women recover without having recourse to any so-called *styptic* measures, and, judging from his past experience in the management of post-partum hæmorrhage cases, he failed to see the necessity for having recourse to the injection of the perchloride of iron as recommended by Dr Barnes, and as practised in so many recently published cases. He could not help entertaining his very strong conviction as to undue credit being often given to the means which may happen to be used in restraining flooding immediately or soon after parturition, and he believed that fussy interference in such cases should be studiously avoided while keeping our patients alive, which could be done more quietly, and he held more safely, by *securing uterine contraction* through pressure and other well-known means, apart from transfusion or perchloride injection, both of which had often been lauded as having saved lives which they may in reality have tended to sacrifice. He advised external compression, with internal manipulation of the cervix and interior of uterus. The uterus, however, was sometimes compressed too much; this was liable to cause the detachment of an embolus, which may possibly have been the cause of death in Dr Connel's case. He indeed, in consultation with Dr Niven, saw a fatal case last week with all the constitutional symptoms of extreme hæmorrhage, although the uterus was firmly contracted. The death in this case he believed was due to embolism. He had used ergotine with apparent success on one occasion.

Dr Bell said that it was the general practice, when he was a student in London, to inject vinegar in these cases: inflammation frequently followed.

On the motion of *Dr Young*, the discussion was adjourned.

Part Fourth.

PERISCOPE.

NOTES ON PHYSIOLOGY AND EXPERIMENTAL PATHOLOGY.

By John G. M'Kendrick, M.D., F.R.S.E.[1]

1. Studies on Biogenesis. By William Roberts, M.D., Manchester. (*Philos. Trans. of Royal Society of London*, vol. 164, p. 2. Read 16th April 1874.)—This paper is a valuable contribution to our knowledge of the origin of bacteria and torulæ. There are at present two rival theories regarding the origin of these minute organisms, namely, panspermism and abiogenesis. The first theory asserts that bacteria and torulæ are developed from pre-existing minute spores or germs; the second, that they may originate *de novo* under certain conditions. Dr Roberts remarks that these two

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[1] These notes will be continued from month to month.

theories are not logically opposed. While the common mode of origin is from spores or germs, it is quite conceivable that abiogenesis may occur in some rare conditions. After an elaborate series of experiments, Dr Roberts comes to the following conclusions as to the conditions under which organic mixtures and liquids are rendered barren by heat:—1. Organic mixtures and liquids are capable of being permanently sterilized by the heat of boiling water. Some are sterilized by an exposure to this heat for a few minutes, others require an exposure of twenty to forty minutes, and others an exposure of one, two, or several hours. A slight difference in the aggregation of the materials, or a slight difference in their reaction, was sufficient to alter very greatly the amount of heat required for their sterilization. 2. The degree of heat was compensatory to the duration of its application; that is, a longer exposure to a lower temperature was equivalent to a shorter exposure to a higher temperature. 3. Slightly alkaline liquids were always more difficult to sterilize than slightly acid ones. 4. No organic liquid or mixture, subjected for however short a time to the heat of boiling water, ever produced (provided there was no fresh infection) any fungoid or torulaceous organism. If germination took place, the organisms were invariably bacteria. 5. Sterilization generally proved permanent if germination did not occur within four days. Sometimes germination took place on the sixth or eighth day, and very rarely ever as late as the tenth or twelfth day.

Dr Roberts next proceeds to investigate the question of the capacity of the juices and tissues of animals and plants to generate bacteria and torulæ without extraneous infection. He experimented on egg albumen, blood, urine, blister serum, milk, grape juice, orange juice, tomato juice, turnip tissue, and potato tissue. The materials of the experiment were enclosed in sterilized glass tubes or bulbs, which were plugged at one end with cotton-wool, and hermetically sealed at the other. They were then set aside in a warm place to see if they would germinate. The results were, that out of 90 such experiments, in 67 there was absolute sterility, while organisms appeared in 23. From these experiments, Dr Roberts draws the general conclusion that the normal tissues and juices have no inherent power to originate organisms, and that when organisms appear therein their development is due to germs imported from without. He then deals with cases in which bacteria make their appearance in liquids which have been subjected to a boiling heat. Bastian and Cohn aver from their experiments, that the degree of vital resistance to heat of bactéria and their germs varies according to the nature of the liquid, and possibly also to the species of bacteria. Roberts shows that alkalized hay infusion will germinate after exposure to 212° F. for an hour, but sterilization is effected if the heat be continued for two hours. He also shows, by 21 experiments, arranged comparatively, that in ordinary air and water particles exist which, in certain liquids, are capable of germinating after exposure to 212° F. for five minutes. One of the most striking facts ascertained in these experiments is that, while unneutralized hay infusion becomes sterile after exposure for five minutes to a boiling heat, if a little ammonia or potash be added, so as to render it slightly alkaline, it may be boiled for one hour, and still be capable of germination. How does the alkali act in this case? Does it increase the vital resistance of the germs to heat, or does it increase "the abiogenic aptitude" of the infusion? This question was answered by ten very ingenious experiments. Ten flasks were charged with unneutralized hay infusion, boiled over a flame for five minutes, and plugged with cotton-wool. At the end of a fortnight, no germination had occurred. Ammonia gas was then permitted to pass through the cotton-wool in five of the flasks, while liquor potassæ was introduced into the fluid in the other five by breaking a small hermetically-sealed tube containing this fluid which had been introduced into these five flasks at the commencement of the experiment. In all the ten flasks the fluid was now alkaline, but no air had been admitted. They remained sterile, proving that the presence of alkali did not lead to germination, although the flasks had been boiled for only five minutes. When the flasks were unplugged, and air or water admitted, germination occurred in every instance in a few days. It appears, therefore, that the presence of alkali in these fluids increases in some unexplained way the vital resistance of bacteria to heat. In conclusion, Dr Roberts says—"Nevertheless, I cannot withstand the impression that this general and common mode of origin (panspermism) may, under rare conditions, be supplemented by another and an abiogenic mode of origin." After numerous experiments made in 1869, 1870, and 1871, the reporter came to the same opinion. With our present knowledge, it is unphilosophical to assert that abiogenesis never occurs. These researches of Dr Roberts are of the highest importance at the present time, when the rôle played by these little organisms in disease is beginning to be recognised.

2. ON THE INFLUENCE OF EXTIRPATION OF THE EYEBALLS UPON THE COLOUR OF CERTAIN ANIMALS. By M. G. POUCHET. (*Robin's Journal de l'Anatomie*, Sept., Oct., p. 558.)—In July 1873, M. Pouchet visited an aquarium in Vienna, and had his attention directed to a sole which was much darker in colour than any of its comrades in the aquarium. He found on examination that the fish had double cataract; and in Stricker's laboratory he performed numerous experiments on fish, to ascertain whether or not there was any connexion between vision and the colour of the skin. He found invariably that ablation of the eyeballs caused permanent dilatation of the black chromoblasts of the skin, so that fish which had been subjected to this mutilation could be readily known from others in the same aquarium, and exposed to the same amount of light. He offers no explanation of this curious result. It agrees h the fact, that fishes and amphibians living in subterranean

waters have weak organs of vision, and are of a dark colour. On the other hand, it does not harmonize with the observation that terrestrial animals having weak eyes, or none (with the exception of the mole), are usually light in colour.

3. THE INFLUENCE OF THE SKIN ON THE URINARY SECRETION. By DR KOLOMAN MULLER. (*Archiv für Experimentelle Pathologie*, 1873.)—This is an experimental research to determine how far the amount of urine excreted may be affected by applying to the skin cold, heat, friction, irritation by epispastiques, or layers of varnish. The following are briefly the results:—1. Cold increased the flow; 2. Heat diminished the flow; 3. Friction and epispastiques had no effect; 4. Varnishes caused a considerable diminution. This last effect is not what one would expect, but it may be accounted for by the fact that varnishes cause great dilatation of the vessels of the skin.

4. A NEW METHOD OF PRODUCING ARTIFICIAL DIABETES. By C. A. EWALD. (*Centralblat*, 1873, No. 53.)—Ewald has found that when nitro-benzoin is introduced into the blood of rabbits, sugar quickly appears in the urine to the extent of from 1 to 2 per cent. In dogs, large doses of nitro-benzoin introduced into the blood were not followed by diabetes, but a dose of from 7 to 10 grains put into the alimentary canal produced it within an hour. It is interesting to know that the reporter has found nitro-toluol, an allied substance, to have a similar effect.

5. ON THE INFLUENCE OF THE BRAIN ON THE TEMPERATURE OF THE BODY. By J. SCHREIBER. (*Pflüger's Archiv*, 1874, Bd. viii.)—Tscheschichin first observed, experimentally, that there is somewhere in the brain a centre which regulates the temperature of the body. His conclusions have been called in question, notably by Heidenhain. The present research by Schreiber shows that when any part of the pons varolii, the cerebellum, the central peduncles, or the hemispheres, is injured, there is a diminution of the temperature of the rectum (1°–6°), if the animal be placed in an atmosphere the temperature of which is relatively low; but if it be kept warm, either by being placed before a fire or by being wrapped up in cotton-wool or flannel, there is an increase of from 1°–3°. Schreiber therefore supposes that injuries do not diminish the production of heat, but simply place the animal in a condition in which there is greater difficulty in maintaining a normal temperature. On the other hand, Schreiber found that an injury inflicted on a narrow area, separating the upper limit of the medulla oblongata from the pons, was always followed by a permanent increase of 3°–5°, even when the animals were placed in a cold atmosphere. These researches are of great interest, as bearing on the pathology of fever, and they support the views long advocated by Professor Laycock as to the existence of thermal centres.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario College of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

APOMORPHIA.—Under the guidance of Prof. Kohler, Max Quehl has performed a series of experiments with this drug. The following are some of his conclusions:—It affects neither the motor nor the sensory nerves. There is no evidence of any influence upon the vaso-motor nerves, nor of any paralyzing influence upon the reflex action of the sensory nerves upon the vaso-motor centre through the spinal cord. Quehl regards apomorphia in small doses as the best and least dangerous of all emetics. In the discussion on Quehl's paper at the Leipsic Society of Natural Science, Reigel stated that he had seen vomiting induced by apomorphia after section of the vagus. He considers that the vomiting is due to the action of the drug on the central nervous system, for after a series of sections of the spinal cord he at length arrived at a plane, after which no vomiting occurred.—*Med. Chir. Rundschau*, Heft 2, 1874. The experiments which M. David performed with apomorphia are recorded in the *Comptes Rendus*, August 24, 1874. He found that a solution of from half to two milligrammes subcutaneously injected produced vomiting in a dog in four or six minutes, preceded by very slight nausea. Vomiting in man was produced by from three to four milligrammes. There was slight uneasiness and vertigo before emesis was produced. The vomiting came on suddenly, and was renewed three or four times; recovery soon followed. M. David found that apomorphia produces in certain animals, as cats, pigeons, rabbits, rats, and guinea-pigs, a special excitement, which he attributes to a specific action on the nervous centres.

ATROPIA AS AN ANTIDOTE TO POISONOUS MUSHROOMS.—Dr T. Lauder Brunton has a very interesting paper on this subject in the *Brit. Med. Journal* for 14th Nov. 1874. He says that one of the most perfect instances of antagonism with which we are acquainted is the power of atropia to counteract the poisonous principle of mushrooms. The active principle of the *Agaricus muscarius* or *Amanita muscaria* was separated by Prof. Schmiedeberg of Strasburg, and named by him muscarin. The merest trace of this alkaloid will arrest the pulsations of the frog's heart almost instantaneously; but if a minute quantity of atropia be brought into contact with the organ it will begin to pulsate again. A little atropia at once counteracts the effects of muscarin on the heart in mammals, just as it does in the frog. Brunton discovered that muscarin causes contraction of the pulmonary vessels, and so gives rise to the intense dyspnœa so characteristic of this poison. The dyspnœa, as well as the other symptoms of poisoning, disappear almost immediately after the injection of atropia.

In cases of poisoning by mushrooms, the stomach should be emptied first; and it is curious that in such cases tickling the fauces is much more efficacious in producing vomiting than the administration of tartar emetic. The antidote may be given by the mouth, either in the form of tincture of belladonna or liquor atropia; but Schmiedeberg and Koppe prefer subcutaneous injection, on account of the more rapid absorption and speedy action of the drug, as well as the more accurate adjustment of the dose. The dose for subcutaneous injection should be about one-hundredth of a grain, or about one minim of the liquor atropiæ sulphatis (B.P.), repeated, if necessary, until the dyspnœa is relieved.

BOLDO.—This drug, which has recently attracted much attention, was first described in 1782 by Molina as Peumus boldus N.O. Monimiaceæ. The leaves are the part used, on account of the aromatic oil which they contain. The drug has not yet been used in this country, but in France it has been experimented with by Dujardin-Beaumetz and Claude Verne. It has been found to act as a stimulant to digestion, and it exerts a powerful influence on the liver.

The leaves when fresh are green, but change on drying to a reddish brown; they are opposite, entire, and oval; they have an aromatic taste and odour, are coriaceous in texture, and are covered on their surface with small glands.

Boldo is indigenous to the New World, and has not been met with outside Chili. It would be beyond the scope of this "Report" to enter into a lengthened description of this plant, but full particulars are given in M. Verne's exhaustive essay—a summary of which appears in the *Pharmaceutical Journal* for 21st Nov. 1874.

The most abundant product yielded by the plant is the essential oil, as much as two per cent. having been obtained. An alkaloid termed *boldine* has also been discovered by M. Verne in connexion with M. Bourgoin. In the Hôtel Dieu and Hôtel Boujon various preparations of boldo have been tried, such as the alcoholic extract, the aqueous extract, the essential oil, tincture, wine, and syrup. The cases in which it has been chiefly employed are those of atony of various organs, when the patients could not tolerate quinine. Beaumetz commences the administration of boldo with fifty centigrammes of an alcoholic tincture made by macerating 100 grains of the bruised leaves in 500 grains of 60° alcohol and filtering. This dose may be increased to two grains daily, but large doses produce vomiting.

ON THE ELIMINATION OF CHLORATES.—M. Rabuteau concludes from his experiments that chlorates of potash and soda, which have penetrated by absorption into the blood, are eliminated by the kidneys without undergoing any decomposition.—*Gaz. Med. de Paris*, 28th Nov. 1874.

THE PHYSIOLOGICAL ACTION OF THE CHINOLINE AND PYRIDINE BASES.—The *Medical Times and Gazette*, 28th Nov. 1874, contains a very interesting paper on this subject by Drs John M'Kendrick and James Dewar of Edinburgh. Quinine, cinchonine, or strychnine, yield, when distilled with caustic potash, two homologous series of bases, named the pyridine and chinoline series. Bases isomeric with these may also be obtained by the destructive distillation of coal, or from Dippel's oil, got from bone. Greville Williams has pointed out that chinoline obtained from coal-tar differs in some respects from that yielded by cinchonine.

The following are the general conclusions arrived at by the authors:—

1. There is a marked gradation in the extent of physiological action of the members of the pyridine series of bases, but it remains of the same kind. The lethal dose becomes reduced as we rise from the lower to the higher.

2. The higher members of the pyridine series resemble in physiological action the lower members of the chinoline series, except (1) that the former are more liable to cause death by asphyxia, and (2) that the lethal dose of the pyridines is less than one-half that of the chinolines.

3. In proceeding from the lower to the higher members of the chinoline series, the physiological action changes in character, inasmuch as the lower members appear to act chiefly on the sensory centres of the encephalon and the reflex centres of the cord, destroying the power of voluntary or reflex movement; while the higher act less on these centres, and chiefly on the motor centres, first as irritants, causing violent convulsions, and at length producing complete paralysis. At the same time, while the reflex activity of the centres in the spinal cord appear to be inactive, they may be readily roused to action by strychnine.

4. On comparing the action of such compounds of C_9H_7N (chinoline) with $C_9H_{13}N$ (parvoline, etc.), or $C_8H_{11}N$ (collidine) with $C_8H_{15}N$ (conia from hemlock), or $C_{10}H_{10}N_2$ (dipyridine) with $C_{10}H_{14}N_2$ (nicotine from tobacco), it is to be observed that the physiological activity of the substance is, apart from chemical structure, greatest in those bases containing the larger amount of hydrogen.

5. Those artificial bases which approximate the percentage composition of natural bases are much weaker physiologically, so far as can be estimated by amount of dose, than the natural bases, but the *kind* of action is the same in both cases.

6. When the bases of the pyridine series are doubled by con-

densation, producing dipyridine, parapicoline, etc., they not only become more active physiologically, but the action differs in kind from that of the simple bases, and resembles the action of natural bases or alkaloids, having a similar chemical constitution.

7. All the substances examined in this research are remarkable for not possessing any specific paralytic action on the heart likely to cause syncope; but they destroy life either by exhaustive convulsions or by gradual paralysis of the centres of respiration, thus causing asphyxia.

8. There is no appreciable immediate action on the sympathetic system of nerves. There is probably a secondary action, because after large doses the vaso-motor centre, in common with the other centres, becomes involved.

9. There is no difference, so far as could be discovered, between the physiological action of bases obtained from cinchonine and those derived from tar.

Toxic Action of Flowers of Colchicum Autumnale.—M. Pierre has recently made a communication on this subject to the Académie de Médecine. He remarked that the fully expanded flowers will stain the skin a livid greenish yellow colour, like that of a corpse in a state of incipient decomposition. He is of opinion that it is principally during the period of fecundation that the colchicum possesses this property, and he attributes it to an extremely volatile substance which has not yet been studied. In certain localities where saffron is largely cultivated, labourers employed in plucking the flowers not unfrequently suffer from symptoms of poisoning, and present a peculiar swollen and bloated appearance. M. Pierre's communication is given in a recent number of the *Comptes Rendus.*

Croton-Chloral.—The *Journal de Pharmacie et de Chimie*, vol. xx. p. 278, contains a communication by Dr R. Engel on this substance. Croton-chloral, or trichlorocrotonic aldehyd, $C_4H_3Cl_3O$, was first obtained by Krämer and Pinner by passing a stream of chlorine into aldehyd for twenty-four hours.

Anhydrous croton-chloral is a colourless oleaginous liquid, insoluble in water, but combining with it to form a hydrate which crystallizes in white nacreous spangles. The hydrate is more soluble in warm than in cold water, and is very soluble in alcohol; it is more soluble in glycerine than in water. According to Liebreich, croton-chloral acts as an hypnotic; but, unlike chloral, its use is not followed by lowering of pulse and respiration. Under the influence of caustic potash, croton-chloral breaks up into allylchloroform and formiate of potash; but allyl-chloroform being very unstable, rapidly decomposes into hydrochloric acid and bichlorallylene. It is to this latter substance that Liebreich attributes the action of croton-chloral. He asserts that while trichlorinated substances, as chloroform and chloral, act on the brain, spinal cord, and heart, the bichlorinated substances, as bichloride of ethylene and bichlorallylene, act only on the brain and spinal cord. In cases of facial neuralgia, Liebreich has employed the croton-chloral with great success, the pain often ceasing before production of sleep. Croton-chloral acts as a hypnotic in doses of from half to one gram.

CROTON-CHLORAL HYDRATE IN MEGRIM.—Professor Sydney Ringer has a paper on this subject in the *British Medical Journal*, 21st Nov. 1874. The pain of megrim is situated in the fifth nerve, and remembering how closely allied megrim is to facial neuralgia, he has employed it in the former affection with satisfactory results. In those cases which are accompanied by severe vomiting and retching, croton-chloral is useless, being speedily rejected. Professor Ringer writes—"It seems to me that in many instances two kinds of headache coexist, one sometimes predominating, sometimes the other. One appears due to affection of the cutaneous nerves, and is generally accompanied by tenderness. Patients describe the other as a 'stupid headache,' 'a feeling of bewilderment,' 'a bewildering headache.' After dispersion of the first form by croton-chloral, the stupid headache often continues, but may ordinarily be relieved by bromide of potassium. Indeed, in many cases I have found it useful to combine these remedies."

GOA POWDER.—In a paper in the *Medical Times and Gazette*, 24th Oct. 1874, Dr Fayrer draws attention to this remedy for Indian ringworm. A few grains of the powder are dissolved in vinegar or lemon-juice to about the consistency of cream, and this is painted over the eruption and for some little distance beyond its margin on the sound skin. It causes no pain at first, but in a few hours there is a dull, heavy sensation, as though the skin had been bruised, the eruption becoming white, whilst the surrounding skin is stained a dark brown. The sense of uneasiness soon passes away, the integument re-assumes its natural character, and all traces of the disease disappear. According to Mr D. S. Kemp (*Pharm. Jl.* 1864) orchella weed (lichen orcella) exported from the coast of Africa, north of Mozambique, to India, is the most probable source of Goa powder.

Dr Fayrer concludes—"Whatever it may be, there can be no doubt of its efficacy in the treatment of the skin diseases I have alluded to, and I should think most probably it might be useful in others also. I venture therefore to commend it to the notice of dermatologists, and to hope that it may be imported into this country, and that its efficacy may be further tested in the treatment of skin diseases."

In the *Medical Times and Gazette*, 14th Nov. 1874, Mr George Gaskoin also writes on the subject of Goa powder. The preparation which he employs he obtains from Mr Garrad of Leamington; it bears the Government stamp of the Bombay Presidency, and is sold by Messrs Kemp and Co. of Bombay and Poonah. Mr Gaskoin states that, on the whole, his experience is such as not to lead him to praise the drug. He says that it gives an ugly brown stain, which is followed by an amount of inflammation which equals that from the coarsest remedies.

THE PHYSIOLOGICAL ACTION OF SALICYLIC ACID.—The *Archiv der Pharmacie*, vol. v. p. 445, gives Prof. Kolbe's researches. The fact that salicylic acid could be easily prepared from carbolic acid and carbonic anhydride, and that it could be again decomposed by heat into the same bodies, led Prof. Kolbe to think that it might prevent fermentative and putrefactive processes, and act as an antiseptic. Many experiments have proved that salicylic acid does act powerfully as an antiseptic. Prof. Thiersch found that, as a powder alone, or mixed with starch, it will destroy the fœtid odour of cancerous surfaces, etc., without giving rise to inflammation. A solution of 1 part of the acid and 3 parts of sodium phosphate in 50 parts of water promotes the healing of granulating surfaces.

EUCALYPTUS GLÓBULUS.—The "Revista Medica de Chile," for the month of October last, quoting from the "Eco de Cordova," gives the following interesting particulars concerning this plant:—There have been planted in California, during the last few years, not fewer than a million of Eucalyptus Glóbulus, tree so famous for drying marshy ground, and at the same time destroying malaria. The heart of this tree is a very nutritive and much appreciated food. There are also obtained from it wine, vinegar, a kind of sugary substance, and a large quantity of starchy matter like sago, of which it possesses the taste and properties. The wood of the trunk is used for making musical instruments, tubes, and pumps. The tender and fibrous matter of the leaves can be used instead of cork. The pulp of the fruit is very agreeable, the kernel very mucilaginous. When toasted and powdered, many persons in the interior use it instead of coffee.

The trunk yields also a kind of flour like maizena, and a whitish liquid, similar to that which contains the fruit known under the name of cocoa of Panama. The dried stalks are used for making mats, hats, baskets, and brushes, and are exported in considerable quantity to Europe, where they are employed in the manufacture of fine hats, in which form they are partly sent back to Brazil. The material exported, and that used for home consumption, is valued at nearly 2,820,000 dollars. The leaves yield a kind of wax, which is used for the adulteration of wax candles, of which there is a great consumpt in the northern provinces, principally in Cearó, when it constitutes an important branch of commerce.

The annual export of this wax is reckoned at more than 880,000 kilogrammes, the home consumption over 734,000, and the value of the annual production at least 4,220,000 francs.

Part Fifth.

MEDICAL NEWS.

OBITUARY.

The Late Thomas Wormald, F.R.C.S.—We are reminded of having neglected to allude to the death of this well-known surgeon at the time when it occurred, by perusing an excellent sketch of his life and character in the volume of Reports of St Bartholomew's Hospital, recently published. We consider it a happy thought, successfully carried out in several instances already, to record in that work the services of men who have deserved well of the Institution, such as William Lawrence, Henry Jeaffreson, and now Thomas Wormald. The last named, whom we knew intimately in former days, has been drawn to the life, by more than one friendly hand, in the short memoir referred to; and we heartily endorse what has been said in his favour, both as a man and a teacher. As a practical surgeon, we did not know him, although in those early days of which we speak, when Stanley taught systematic anatomy, and Skey and Wormald were his demonstrators, he (Wormald) seemed to possess all the qualities which go to the making up of a great surgeon. He was a pupil, probably the last, of John Abernethy, and was said to imitate him in many things. There may be some ground for this allegation; but we are rather inclined to think, that there was a natural likeness in the mental constitution of the two men. Wormald was undoubtedly a man of original genius, and used to say things which no power of imitation could produce. It is your commonplace men who imitate, and that only in manner, in voice, in dress, or other outward peculiarities.

We notice one error as to dates in the memoir. Mr Skey was associated as demonstrator with Mr Wormald much longer than the writer supposes. They acted together, and with Stanley, until the session 1834–5, at the commencement of which, Skey joined the Aldersgate School of Medicine. It is a remarkable and melancholy fact, that all of these men were about sixty years of age before they had wards of their own in St Bartholomew's. This helps to account for Wormald being less known by the general public than his sterling merits deserved; but, moreover, he had independent means, enjoyed country occupations and sport, and therefore never made a toil of his profession. His character was an estimable one; he had a firm faith in revealed religion, and died at the ripe age of seventy-one, in the last week of December 1873.

The Late Francis Kiernan, F.R.C.S., F.R.S.—This distinguished anatomist, who forty years ago gained for himself a very high place among those who have added to our reliable knowledge of minute structure, died in London, on the last day of 1874, he age of seventy-four, twelve months after his contemporary

Thomas Wormald. Born within two years of each other, they were both students at Bartholomew's, and both passionately devoted to anatomy; but their histories were materially different. Kiernan was not the apprentice of a Hospital Surgeon, and therefore, in accordance with the use and wont of those days, was ineligible as a teacher in the School or as an aspirant for a surgical appointment. On his return from foreign travel and prolonged study in various Continental schools, he gave private demonstrations in anatomy in Charterhouse Square, and speedily became very popular among the students, who discovered both the intrinsic and the relative value of his instructions. It would appear, however, that influence was brought to bear, by interested parties, on the Council of the College of Surgeons, who passed a resolution which had the effect of rendering useless his certificates of attendance. He petitioned the College to rescind their resolution, and, much to the credit of Mr Vincent, Mr Lawrence, and Mr Lloyd, all members of the Hospital staff, they backed him with a strong testimonial as to his pre-eminent qualifications, while Lawrence added the powerful advocacy of his keen and polished eloquence. It was in vain. Kiernan abandoned teaching and devoted himself to those researches into the anatomy of the liver, which were so complete in themselves and so suggestive, in regard both to structure and function, that subsequent inquirers have found little to investigate. Forty years have passed since his paper was published in the Philosophical Transactions. It obtained for him at once the honour of being elected a Fellow of the Royal Society, and, not long afterwards, the Copley medal, when his original paper was expanded into a separate work, entitled, "Anatomical Researches into the Structure of the Liver."

Our personal acquaintance with Mr Kiernan dates as far back as that period of his life, 1834, when it effloresced so richly and bore such ample fruit; but we have not heard of any subsequent indications of the same intellectual activity. How often is this exemplified in the cultivators of science! A great and sustained effort for a few years in early life, a sudden and splendid season of flowers and fruit, and then a long and perhaps final period of apparent exhaustion and barrenness. But Kiernan was not otherwise idle, and did good service in the Senate of University College, in the Council of the Royal College of Surgeons, and latterly as a member of the Board of Examiners.

He had a severe and tedious illness in 1850, during which he was assiduously attended by his friends Hodgson, Skey, and Stanley, proving that all old causes of discomfort had been forgotten on both sides, since those early days when the Bartholomew influence had been exerted against the young aspirant in Charterhouse Square.

Mr Kiernan had a fine intellectual eye, and well-formed head, with a square ample brow, resembling that of the first Napoleon. This recalls an observation which he once made in our hearing, and which deserves being recorded as coming from a man of his acute-ness and accuracy. On going to Paris to complete his studies, he left behind him a hat which fitted him comfortably; but after a period,—we think twelve months,—of very intense mental exertion, he found upon his return home, that the hat would no longer receive his head. His inference was, and we think he had grounds for it, that the brain had grown and the calvarium had expanded in proportion.

We conclude this brief notice, by recommending those who have learned the structure of the liver merely from lectures or handbooks, to read Mr Kiernan's original paper. It is a model, in every respect, of thorough, conscientious work.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

SIR,—In the review of Professor Aitken's *Outlines of the Science and Practice of Medicine* in your last issue, the following passage appears, in reference to the Army Medical School:—

"A school which sets itself above all others by assuming the right to examine and reject, if in its opinion unqualified, the licentiates of all other schools, not only in some branches peculiar to itself, but in all the branches of medical science."

The professors of the Army Medical School "assume" no such power. The candidates who come to Netley to undergo a course of special instruction for the Medical Services of the British and Indian Armies and the Royal Navy, compete at an open examination in London for their appointments, before a board of independent examiners, appointed by Government for the purpose, over whose proceedings the Army Medical School has no authority at all.

The examinations at Netley are confined entirely to the special subjects taught there, and the power of conducting them is not "assumed," it is conveyed to the professors by a special warrant. The School has been in existence for more than fourteen years, and so carefully have candidates been sifted by the London board, *acting independently of the School*, that in twenty-nine terms the rejections have not in all exceeded two, at the outside three.

Surely the reviewer can hardly be ignorant that for at least a century, Government has exercised the right of examining candidates for the Medical Services of the State, no matter from what schools they have come.

I have only to add, that the examining board in London have more than justified their rejections by the extracts from examination papers produced before the Medical Council.—I enclose my card, and am, Sir, your obedient servant, M.D.

12th January 1875.

REPLY TO ABOVE.

To the Editor of the Edinburgh Medical Journal.

SIR,—Any little mistake I may have made in regard to what is "assumed" by the Netley School, and what is thrust upon them by "special warrant," is of little consequence, and does not alter the fact that a Government Professor, in what may be called the finishing school for Army Medical probationers, has put forth a work for the instruction of medical students in practice of medicine, which is lamentably defective and incorrect. Our students naturally desire to pass creditably, and I have advised them to possess themselves of this book, as the only means of acquainting themselves with views which, so far as I know, are taught nowhere else, but which, issuing from such a source, are presumed to be of importance in qualifying for appearing before any Board for the examination of candidates for the Army Medical Service. If the London Board of independent examiners think otherwise, perhaps they, or M.D. for them, will say so. The reply cannot but be of importance.—Yours obediently, THE REVIEWER.

INVERSION OF BLADDER.

To the Editor of the Edinburgh Medical Journal.

SIR,—It has been my lot to see, and to some extent to treat, a very rare, if not a unique case; and, as I have repeatedly been advised by professional friends to publish it, I now send a few notes for insertion in the Journal if you have space for them.

A female child, aged sixteen months, apparently strong and healthy, was shown to me by the mother a short time ago.

On examination, I found the condition of parts to be as I shall now attempt to describe.

When I separated the labia majora, what I, and two others to whom I have exhibited the case, considered to be the cervix uteri, presented itself about midway between the anus and the orifice of the urethra or outlet of the bladder. I say, or *outlet of the bladder*, for if there be urethra, the orifice was so large that with little difficulty I could have passed the extremity of my little finger through it into the bladder. At the apex of the cervix uteri, or what may at least be said to represent it, is a small depression, which I at first supposed to be the os uteri; but on attempting to pass a probe, the instrument did not enter even to the extent of, I may say, the sixteenth part of an inch. Therefore, I concluded that either the os is impervious, or it is not the os uteri at all.

What favours the view that it is uterus (though imperfectly developed), is the curious coincidence of there being what appears to be half of a *cleft vagina* adhering to the inner surface of each labium. And when the labia are approximated, the cervix uteri, if such it may be called, is just, as it were, between the upper ends of the cleft vagina.

The child was taken, at my desire, to one of the most eminent obstetric physicians, and he at first thought it was one of extroversion of the bladder, but after more careful examination, he acknowledged that it was not so.

I then dismissed the case for the time being, telling the mother that when the child is older, something might perhaps be done for her. But sooner than I expected, and only a week or two after I had last seen the child, the mother brought her to me again, saying that on the previous day "a red swelling" had made its appearance in the upper part of the genital fissure, and that an hour or two before she came to me, the tumour suddenly increased in size.

The child had not slept all the previous night, and was crying when I saw her.

The appearance now presented (when the genitals were uncovered) was totally different from what I had previously observed at the point corresponding with the urethra. A red, somewhat globular, elastic swelling, about the size of a small orange, now existed there. The child was suffering considerably from "tenesmus;" fæcal matter was repeatedly passed from the bowels, and the abdominal muscles contracted at short intervals, just as they do in the second stage of labour, I thought.

If I had not previously seen the child, I would now have been puzzled as to the nature of the case before me; but, as it was, I had no doubt. Yet, in order to make doubly sure, I manipulated with a probe so as to ascertain whether the whole, or a part only, of the bladder *had now become extroverted.* I pushed the tumour to one side a little, and, placing the end of a probe close to its point of attachment, tried to find a passage into bladder; but no such passage now existed; whereas, prior to the extroversion, a probe readily passed into its cavity—which appeared to be of fair size.

I now endeavoured to re-turn the bladder, per urethram, into the pelvis, but failed. The tenesmus and straining, or "bearing down," of the child increased, or, at least, was renewed, with every attempt I made. I then put the child under the influence of chloroform, and again made the attempt.

At first I kept up steady pressure on the tumour with both hands, or rather with the fingers of both hands, then maintained the pressure as well as I could with my left hand alone, while, with the index finger of my right one, I pushed the lower portion of the tumour backwards. By manipulating in this manner for a short time, I got part of the bladder through the dilated urethra, or at least neck of bladder, into the pelvis.

Seeing now that I was likely to succeed in accomplishing the reduction, I renewed my efforts still more vigorously, and, all on a sudden, up went the rest of the extroverted bladder, through the dilated urethra into the pelvis.

A soft pad and T bandage were then applied to prevent extroversion, or avoid the risk of its return so long as the contractions of the abdominal muscles continued.

A few days subsequently I called to see the child; the pad and bandage were still retained, but, when necessary, they were pushed a little to one side. The mother told me that the child had been suffering from diarrhœa, and that while the bowels were being evacuated, the child strained greatly, but was in other respects as well as usual.

Several weeks have now elapsed, and I have not been required again, so that I conclude the child is in her ordinary health. —I am, Sir, your obedient servant,

SOMMERVILLE OLIVER, M.D.

It may be well to supplement what I have stated by mentioning that though, when the child strains a little, as in the act of passing water, a small portion—about the size of a "fourpence piece"—of the mucous membrane of the bladder presents itself at the urethra, or "outlet of the bladder," yet that the "sphincter vesicæ" remains intact, may, I imagine, be inferred from the circumstance that the child can and does retain her urine for an hour or two at a time.

I may also add that the extroverted bladder was so slippery that, when trying to reduce it, I was obliged to cover it with a piece of cotton cloth. S. O.

33 Broughton Place, Edinburgh,
31st December 1874.

INTRAVENOUS INJECTION OF AMMONIA.

To the Editor of the Edinburgh Medical Journal.

SIR,—Would you kindly insert the following short note of a case under the care of Mr Joseph Bell in this hospital, which illustrates the very marked effect of the injection of ammonia, and thus oblige, yours, etc.,

HOLLAND J. COTTON, M.B., etc.,
House-Surgeon, Edin. Royal Infirmary.

Robert M'Nair, æt. 47, was admitted at 9 P.M. on 9th January 1875, with both his arms mangled and severe head injuries, caused by a railway accident near Bathgate. Mr Bell on his arrival at once amputated both arms below the shoulder, dressing the stumps and scalp-wounds antiseptically, the whole being concluded by 9.50. At 11.30, the carotid pulse was very small and rapid (140), the patient complaining of nausea and cold extremities. Brandy was given freely and the artificial heat increased, but with no effect, as his pulse became so feeble as to be scarcely perceptible and his heart almost ceased to beat. The patient being at the very point of death, and as a last resource, I, at the suggestion of Dr Gibbons, one of the house-physicians, injected into the internal saphena some ammonia (5♏ of liq. ammon. fort. to 12♏ of water). This was at 12, and about 16 seconds after, the heart was beating more strongly and regularly. The patient's face gradually lost its death-like appearance, and the warmth of the body began to return. Soon he got very restless, raising his stumps and using violent language, so that I ordered morphia, which was repeated several times.

The after-progress of the case was very favourable till the 14th, when his temperature rose to 104·4°, the pulse remaining at 120. The dressings were found putrid, but there were no rigors, the patient however constantly wandering. On the 17th, Mr Bell ordered stimulants, his pulse being 130, and temperature 105°; but he was quite delirious, and his breathing was rapid and laboured, with slight twitching of muscles. At 11.15 P.M. his pulse was 140 and temperature 107·9, and at 11.50 he sank.

TRACHEOTOMY IN EMERGENCY.

To the Editor of the Edinburgh Medical Journal.

MY DEAR SIR,—I am obliged to you for kindly requesting a medical account of my case for publication in the *Edinburgh Medical Journal.*

The facts are something like the following:—

George Comb, a weaver, residing at High Street, on the afternoon of 22d December, while partaking of dinner in an inebriated condition, choked himself with a piece of meat.

A few minutes afterwards I was called in, and found the man struggling for breath (struggles becoming faint), eyeballs projecting, face not so much livid as in a state of general pallor; pulse weak, fast, intermitting, and very irregular.

Without losing a minute, I used long forceps, and then probang, but failed to remove obstruction; and as the patient had evidently made a last movement with his head for breath, and being informed that his pulse was gone, I at once performed tracheotomy in the normal situation beneath the cricoid cartilage, and with the best possible result; a sudden rush of air took place, and pulse and breathing were gradually restored.

Dr Morris, who came in to my assistance, kindly helped me to displace the obstruction downwards. The man has since progressed favourably, and four weeks after operation was able to resume work.

Remarks.—I performed the operation with an ordinary bistoury, and used a pair of scissors to keep trachea open till a tube was procured. The tube was closed with a small cork two hours after operation, and removed next morning. The bleeding was scanty, and the contiguity of tube alone controlled it.—With sincere regards, I remain, dear Sir, yours truly, JOHN W. BELL.

P.S.—Mr Barnet was non-professional assistant at operation.

Dunfermline.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Cases in Medical Jurisprudence.* By HENRY D. LITTLEJOHN, M.D., F.R.C.S.Ed., Medical Officer of Health, and Lecturer on Medical Jurisprudence and Public Health, School of Medicine, Edinburgh.

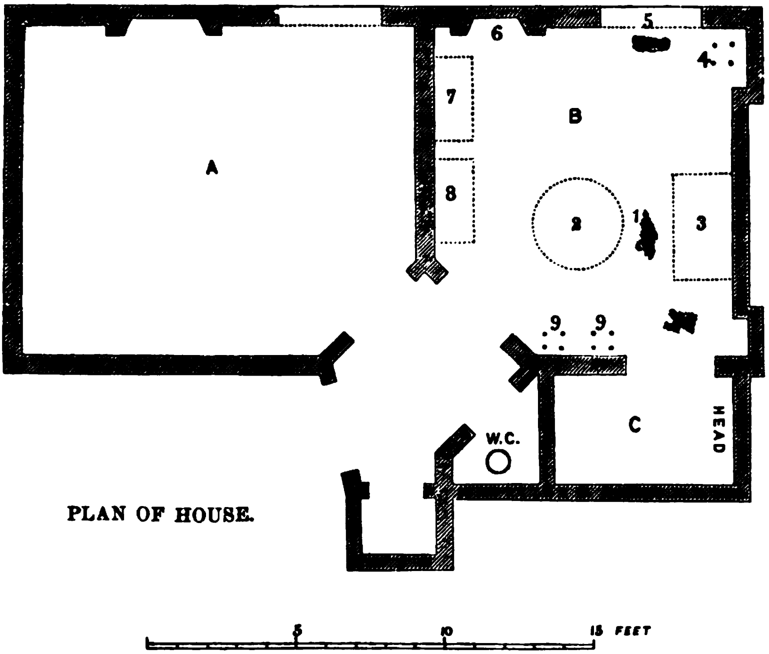
(*Read before the Medico-Chirurgical Society of Edinburgh, 2d December* 1874.)

I. ACCIDENTAL SUFFOCATION, ATTENDED WITH SUSPICIOUS CIRCUMSTANCES.

ABOUT one o'clock on the morning of Thursday, 24th September 1874, a young lad named M'Queen, passing along Ingliston Street, found the body of a man lying partly upon the footpath and partly on the street. The head only was on the footpath, and the arms were extended. There was nothing on the body but a shirt; a glazed tablecover was lying in front of the body, and towards the right side. The body was warm, but the lad felt no movement, and he was sure that the man was dead. M'Queen shouted for assistance, and some police-officers came. By means of their lamps the body was examined more carefully, and the head was seen lying in a pool of blood. Deceased had evidently fallen or been thrown from a height, and on looking up, a window on the third storey was observed with its under sash fully pushed up. At this moment Mr Nevin, the treasurer of the Theatre-Royal came up, and I shall read his statement:—

Patrick Nevin, aged 40, treasurer of the Theatre-Royal, and residing at 4 James Place, Edinburgh, declares,—On the night of Wednesday, 23d September 1874, I was visiting a friend in Roxburgh Street who was unwell, and about half-past one o'clock on the Thursday morning, when sitting in my friend's house, my attention was drawn to hearing the policeman's whistle. I went out, and saw some persons running towards Ingliston Street. I went there, and saw the deceased's body lying partly on the footpath and partly on the street. There were several police-officers there at the time, and I saw that the deceased was dead. The body was lying partly on the right side of the face, and there was nothing on it but a shirt. There was a tablecover lying near it. On looking up to the tenement above, I saw that the window immediately over us, on the third flat, was raised to its full extent. I asked the police to go up stairs with me, and they accompanied me up to the witness Holywell's house. Mrs Holywell opened the door for us, after I had rung the bell. I asked her if she had any lodgers, and she replied she had, and showed me into a room which was dark. I lighted the gas, and observed that there was a quantity of blood on the floor, in a line between the window and the bed. On looking into the bed I found Stoddart lying at the back part of it, with the bed-clothes wrapped about him. I shook him, and he awoke up. I asked him where his companion was, and he replied that he did not know. I saw spots of blood on Stoddart's right hand, and also one or two on the left. The pillow at the front of the bed was also considerably spotted with blood. Stoddart's hands were covered by the bed-clothes, and they appeared to me to be damp. At the time, I thought Stoddart was feigning sleep, and he said no more to me about his companion. I asked him if he had had a quarrel with his companion, and he said he had not, with the exception of some difference on Monday morning about the bed-clothes. I followed the blood-stains to the window, and I saw there was a considerable quantity on the muslin over the window-sill, and on the cover of an arm-chair in the corner. The lower sash of the window was raised to its full extent. There was no looking-glass on the window-sill, but I observed one standing on the top of the trunk nearest the door. There was no blood about the room except on the line between the bed and the window. I examined the looking-glass, which was quite whole, and there was no blood about it. There was an empty tin measure on the table; and lying on the floor, near the window, was a table-knife considerably covered with blood. Criminal-officer Frew came to the house about this time, and Stoddart was sent away to the Police-office. Frew took possession of the room, and locked it up. There was nothing overturned or disarranged in the room further than what I have stated. I observed a damp towel lying on the arm-chair referred to, and I saw there were some slight stains upon it like blood. P. NEVIN.

The house in which deceased lodged consisted of two apartments, both entering from a short lobby. The first, or that next the main door of the house, was the kitchen, in which there was a bed, occupied by the landlady, Mrs Holywell, and her mother, who had come on a visit. The husband in consequence for that night slept in an adjoining house; and at first, when he was found to be absent, some suspicion attached to him. The inner room was a parlour, 11 feet by 10 feet broad, off which there was a dark bed-closet, about 3 feet 9 inches broad by 6 feet in length. There was one window to the street, and the height from the street was about 35 feet. Deceased occupied this parlour and the common bed along with another man of the name of Stoddart. This person was about thirty-five years of age, a native of Scotland. He had been, it was ascertained, in Australia, and since his return had been the inmate of a lunatic asylum. He was a stout, broad-shouldered, rather "dour" looking man. These two lodgers had had a quarrel the previous Monday, deceased complaining to the landlady about Stoddart taking the bed-clothes off him during the night, and insisting on a separation. Mrs Holywell, the landlady, said she preferred deceased as a lodger, and gave Stoddart his leave for the following Saturday. Next day the two had apparently made it up, and finally Stoddart was allowed to remain. The landlady, in her evidence, says—"I never observed any signs of insanity about the prisoner, although at times I thought his look peculiar, and his ways a little eccentric; and that was my reason for thinking it better to allow *him* to leave his lodgings than the deceased." Such were the relations of these two men on the night of Thursday, 24th September. At first sight there were grave suspicions against



PLAN OF HOUSE.

A. Kitchen. B. Parlour. C. Bed-closet.
1. Stain on floor, and knife. 2. Table. 3. Chest of drawers. 4. Arm-chair. 5. Window.
6. Fireplace. 7. Chest. 8. Chest. 9. Two chairs.

Stoddart; and so strongly impressed were the police by them, that they at once apprehended him on a charge of murder. Here is his companion found dead in the public street, having either been forcibly thrown over, or, to escape from some imminent danger, having leaped or fallen from the window. In the room itself, between the bed and the window, there were marks of blood, and in the midst of one of these we have a bloody knife. The pillow-case and bolster at the front of the bed, and also the sheet, were sprinkled with blood. There was also blood on the floor in front of the bed, about midway between the bed and the window, and a considerable quantity on the muslin over the inside window-sill. There was also some on the cover of the arm-chair in the corner, and on the outside of the window-sill. A looking-glass which usually stood at the window had been removed to the top of a chest which lay near the door. The prisoner had the appearance to the witness Nevin of feigning to be asleep, and his hands seemed damp; and the criminal-officer Frew, who more carefully examined him on the spot, says, "His hands were clean, and at the moment it struck me that they had been recently washed."

There was a small quantity of blood on the back of his right hand, and the skin was abraded on the middle of the back of the left hand. The prisoner accounted for this abrasion, saying he had received it at his work two days previously. No struggle or noise, however, had been heard by Mrs Holywell in the kitchen, which was only separated by a brick partition from the room occupied by deceased; and the neighbours in the house adjoining to the bed-closet were unaware of the slightest noise or disturbance during the night.

The first light that was thrown on this puzzling case was by a member of our profession, Dr Joseph Bell, who, in my absence from town, was summoned during the night by the authorities to examine the *locus* and the dead body. Dr Bell was struck by the peculiar appearance of the marks of blood on the pillow, bolster, and sheet. They had not the vivid appearance of unmixed blood, and, from their spray-like arrangement, could only, in Dr Bell's opinion, have been caused by a cough or sneeze acting on blood previously lodged in the air-passages. Several wounds were observed on the head of the deceased, and, from the fact of a bloody knife being found in the room, it was supposed that these wounds might have been inflicted with this instrument. Dr Bell, however, came to the conclusion, from his examination of the blood on the bed, that the marks were not such as could have been caused by blood coming from a wound or a wounded artery, and the wounds on the head had all the appearance of lacerations. Looking to the whole circumstances of the case, the appearance of the room, etc., Dr Bell was strongly of opinion that the hæmorrhage at first was accidental, probably from a phthisical condition of the lungs, or from the bursting of an aneurism, and that deceased, having gone to the window to relieve himself of the blood which threatened to choke him, had overbalanced himself, and fallen accidentally into the street.

Experienced officers in the detective force, and the Procurator-Fiscal himself, did not at first agree in this opinion, and continued to look upon the case as one of grave suspicion.

The following important evidence was subsequently obtained by the police:—

Edinburgh, 29th September 1874.

Ann Farquhar or Lyle, wife of, and residing with, William Lyle, mattress-maker, at 68 Pleasance, Edinburgh, declares,—My house is on the ground flat, and directly opposite to the witness Holywell's house. I am often annoyed during night by people sitting down on the sill of my window and talking loudly. About one o'clock, or shortly after it, on the morning of 24th September 1874, I was awoke by hearing some persons talking loudly and pushing against my window. I arose and drew the blind to one side, and I saw two women and a man standing there arguing. I do not know who they were. There are always a number of loose characters about that district. I cried to them to go away, otherwise I would get the police, and they at once walked up the Pleasance. I stood for a minute looking out, as the morning was very fine. At that moment I heard a clash and a yell. I cannot say which of the sounds reached me first. Just at the moment I observed the shadow of something falling down from Holywell's room window. I did not see the object actually come out of Holywell's room window, but I saw it immediately afterwards. The window was quite dark, and the under sash was raised to the top. I saw no one at the window. I awoke my husband, who is blind, and I told what I had seen. I knew it was a man who had fallen, and I don't think he gave the yell till he struck the ground. My husband told me to put my clothes on and go across the street. I put on a petticoat and a cloak, and went over; but by this time the police and some others were there, and I did not remain. There was no struggle in Holywell's room, and no noise about the room, otherwise I would have heard them. ANN LYLE.

In the course of the morning I carefully examined the room and bed-closet, which, along with the various articles of furniture, had remained undisturbed in the custody of the police. I quite concurred in the opinion expressed by Dr Bell, and elicited the curious fact (which has been noted in the annals of medical jurisprudence), that, with artificial light, stains of blood invisible in ordinary daylight became at once apparent. On examining the bed-closet with a candle more carefully, a large number of spots of blood were discovered, which, without this circumstance, were lost in the general design of the wall-paper and escaped detection. This paper had a cinnamon ground, marled over with small spots of a claret colour.

I may add, that when I conversed that morning with the landlady—an intelligent woman—she was strongly of opinion that Stoddart had murdered the deceased. The examination of the body was therefore looked forward to with great interest. It was made about 11 o'clock in the forenoon of the same day.

Report of Post-mortem Examination.

Edinburgh, 25th September 1874.

We hereby certify, on soul and conscience, that we yesterday, Thursday, 24th September 1874, examined, in the dead-house of the Police-office, the body of a man, which was identified, in our presence, by John Gibson, police-constable Thomas Anderson, and criminal officer William Frew, as that of Andrew Gibson.

External Appearances.—The body was that of a spare but well-formed man, of average height, and apparently about fifty years of age. The face was covered with blood, and the hair of the head on the left side was matted with blood. On moving the head there was evidence of extensive fracture of the skull. On carefully cleaning the scalp, it was found to be wounded in two places on the left side. About three inches above the ear, there was an irregular wound, involving the whole thickness of the scalp, about three inches in length, and running from before backwards. About an inch in front of the ear there was another laceration, the size of a shilling, surrounded by considerable swelling and abrasion. The front of the right knee was abraded. There were the remains of old ulceration on the front of the right leg; and along the outer side of the left thigh, leg, and ankle, there were several limited abrasions.

Head.—Both nostrils contained liquid blood, and blood was evidently escaping from the interior of the left ear. On reflecting the scalp, the whole of the left side of the skull and the under surface of the scalp presented an ecchymosed appearance. The left temporal muscle was extensively broken up and infiltrated with blood. A fracture of the skull (previously felt) was found to pass from the occipital protuberance transversely forwards as far as an inch and a half above the left ear, where it passed directly downwards. The skullcap was now removed, when the right hemisphere of the brain was observed to have a considerable quantity of blood effused beneath the arachnoid on its superior and lateral surfaces. The brain was otherwise uninjured. On removing the organ, the fracture (described above) was seen to pass through the petrous portion of the left temporal bone (detaching a considerable portion of its superior surface), across the base of the skull, and to end in a fine fissure at the outer portion of the right temporal bone. No particular odour was detected in the brain or cranial cavity. The cranium was thicker than usual.

Mouth, etc.—There was a small quantity of blood in the mouth, and blood was protruding in a coagulum from the posterior nares. On opening this cavity, the left middle turbinated bone had its mucous covering ruptured in two places, about the size of millet-seeds, and minute coagula lay in these openings. A considerable quantity of fluid blood was found in the gullet, while in the larynx there were two streaks of blood on either side. In the trachea there was a larger quantity.

Chest.—The heart was examined *in situ*. Its right cavities were moderately distended with fluid blood of a dark colour. Similar blood was found to a small extent in the left cavities (which were well contracted) and in the aorta. These organs were normal. Both lungs were universally adherent to the walls of the chest by adhesions of old standing. On the right side there was a mass of dried exudation, the size of a crown piece. The right lung was healthy, and somewhat congested. On the outer side of the lower lobe of the left lung, the pulmonic tissue, to a depth of fully an inch, was infiltrated with blood, constituting "pulmonary apoplexy." In both lungs, the bronchi, even down to the minutest ramifications, contained blood mixed with mucus. Along the free margins of both lungs there was considerable emphysema.

Abdomen.—In the depending part of this cavity there were several ounces of fluid blood. The liver, which was of normal size, was ruptured in several places—superficially on its upper surface, and more deeply on its under surface close to the vena cava. Patches of extravasation (blood) were seen at several points on the mesentery and on the intestines. The stomach was well contracted, and contained the remains of an ordinary meal, which presented no particular odour. The organ was healthy and uninjured. The symphysis pubis was separated to the extent of one inch, and the pubes on the left side was much shattered. The urethra was torn. Other organs normal.

From the foregoing examination, we are of opinion that death was caused by fracture of the skull and other injuries. These injuries were quite recent, and were such as a fall from a height would produce.

HENRY D. LITTLEJOHN.
JOSEPH BELL.

The foregoing report is drawn up in the usual form, and does not represent the order in which the dissection was conducted. After the examination of the exterior of the body the chest was opened for the purpose of observing whether the source of the hæmorrhage could be traced to an aneurism or to phthisis. The only lesion found was the pulmonary apoplexy, evidently due to the shock of the fall, and in a great measure accounted for by the general adhesion of the pleural surfaces of the lungs. The abdomen was then inspected, and after that the windpipe, throat, and cervical spine. The last cavity opened was that of the cranium, and finally both upper maxillæ were removed, with the result of the detection of the origin of the hæmorrhage.

Our report was supplemented by what is called in Scotland "a precognition," of Dr Bell and myself, in which we gave our opinion as to the significance of facts connected with the case, but which could not be dealt with in our account of the dissection; and also as to the mutual bearing of the circumstances of the case, and the appearances of injury found by us on the body.

Prisoner's Declaration.

Archibald Stoddart, in his declaration before Sheriff Hallard on 24th September 1874, states,—I am 35 years of age, and a labourer. The deceased Andrew Gibson was my fellow-lodger in the house of John Holywell, Ingliston Street. We were not employed in the same work, and only met in our lodgings, where we took onr meals, and slept together in a box-bed, opening from the sitting-room, which we used in common. The window is in the third storey above the street storey. Gibson and I had some slight difference last week about his pulling the bed-clothes on the floor, but that difference was quite made up. We went to bed together last night about ten o'clock, both perfectly sober, and on good terms. I fell asleep, and was wakened by a policeman and another man. By the light of the policeman's lantern, I saw that Gibson was no longer beside me, that there was blood upon the bed and upon the floor of the sitting-room, but I cannot tell how it is that Gibson was not there and that blood was there. I did not in any way interfere with Gibson. I did not assault him or offer him any violence. I had not seen him since I fell asleep last night. If he was found dead on the street, I am not able to account for his death. I had my supper yesterday evening about half-past five o'clock. Gibson came in half an hour later, and then had his. The landlady brought in a tin of water, of which we both drank. There was a knife on the table, which both Gibson and I had used for cutting bread. When I went to bed last night, the looking-glass was standing in the usual place, on a shelf in the sill of the window. The window was shut when I went to bed, but I don't think it was fastened. Gibson slept at the front of the bed, and I at the back.

After a detention of three days the prisoner was liberated; but the inquiries of the police were continued. The case was reported to the Crown authorities, who, on the 10th October, directed no further proceedings.

From the relatives of the deceased it was ascertained that, while ordinarily enjoying good health, he had been subject of late years to occasional and violent epistaxis, the hæmorrhage occurring with great force; and it is in the highest degree probable, that, lying asleep, the hæmorrhage began so suddenly as to cause violent efforts at coughing, thus accounting for the marks on the bed and bedclothes. The blood, continuing to flow with great force and in large quantity, threatened to choke him, and evidently for the purpose of preventing any soiling of the furniture of the room, the deceased (being naturally of cleanly habits, and having on the previous occasion of a similar hæmorrhage shown a great desire to prevent the blood coming in contact with the furniture of the room in which he was) determined to expel the blood, which filled his fauces and pharynx, by vomiting it over the window. At this window, however, there was a wooden shelf covered with muslin, and on the shelf stood the looking-glass. The poor man removed the glass to the chest, and, to save the muslin as much as possible, took hastily from the table the glazed cover, which he placed on the window-sill. In order, however, to clear the shelf, the sill, and the stone of the window, deceased had to get up on his knees, and, in his hurry and excitement, and half kneeling on the slippery tablecloth, he slipped and fell, taking the tablecover with him. The knife had been laid the previous evening on the tablecloth, and when this was removed, fell into one of the pools of blood which were observed stretching in a line from the bed to the window. The presence of some blood, which in the morning was found on the stone sill of the window on the ground flat immediately below the room from which deceased fell, is thus accounted for by deceased having vomited before he fell.

II. ACCIDENTAL SUFFOCATION FROM BURSTING OF AN ABSCESS OF THE TONSIL.

Early in the morning of 30th October, a young married woman was found dead on a sofa in the house of her mother-in-law. Deceased had been married about four months previously, and on the 21st October came to her mother-in-law's house, for the purpose of attending to her in sick-bed. Owing to the limited accommodation no bed was available, and the sofa was used by deceased and her husband. On Tuesday, 27th, she began to complain of sore throat, but otherwise appeared to be in good health. The voice and swallowing were affected, and she got worse on the Thursday night, 29th, and Dr Forbes, Newington Terrace, was sent for about twelve o'clock. He found both tonsils much enlarged. The right one was smaller than the left, and had, according to the friends, recently given way, and discharged a quantity of matter. Deceased appeared to be relieved by this discharge; and after ordering some simple remedies, such as steaming the throat and the application of poultices, the doctor left. The husband and deceased went to bed on the sofa, and, according to his statement, she conversed for some time freely about domestic matters, and about 4 A.M. asked him what o'clock it was, he having got out of bed for the purpose of getting a drink of water. About 6 A.M. she expressed a wish for a drink of water herself, which was given to her, and he returned to bed.

About 7.30, the husband rose and spoke to his wife, but, getting no answer, became alarmed, and called for assistance. On examination she was dead. Dr Forbes was sent for, and found that she had been dead for some little time.

Dr Forbes reported the death to the authorities, but, pending inquiries, returned to say that the mother of deceased averred that deceased had occasionally for the last four years complained of pain in the left side of her chest, and had been cupped at the Infirmary for said pain, and that she (the mother) believed her daughter to have suffered from heart disease, and that the sudden death might thus be accounted for.

On post-mortem examination, there was slight lividity of the face, otherwise quite composed. No marks of cupping could be detected. Internal organs generally healthy. No pregnancy.

Throat.—Both tonsils enlarged, and contained pus, which freely escaped on pressure. Frothy purulent matter in larynx.

Chest.—Trachea and minutest ramifications filled with pus. Lungs congested. Right side of heart distended with dark fluid blood; left well contracted, and nearly empty. Blood throughout body fluid.

These cases are good illustrations of the varying appearances presented in cases of death from suffocation, depending, I believe, on the rapidity of action of the asphyxiating agent, and the condition of the individual as to consciousness at the moment of the accident. In the first case we have a violent effort to prevent impending suffocation; in the second, the death occurred quietly, and without the slightest struggle, for it is difficult to suppose that convulsive movements would not have attracted the notice of the husband, lying as he was in close proximity to deceased. Here the sleep towards early morning may have been deep, and, the woman lying on her back, a sudden gush of purulent matter may in her unconscious condition have caused sudden death. In the other case the man, either awake or only partially unconscious from commencing sleep, is roused by the trickling of blood into the throat and air-passages. Convulsive coughing is induced, sending the blood from the nostrils in the form of spray. Blood continuing to flow accumulates in the pharynx, and necessitates his rising to expel it, and thus prevent suffocation. In his endeavours to avoid soiling the furniture, he ultimately reaches the window, and meets his death in the manner I have endeavoured to describe.

ARTICLE II.—*On the Treatment of Secondary Syphilis.* By J. L. MILTON.

UNDER the head of secondary syphilis, I propose comprising—merely, however, for the purpose of treatment—all forms of infection following upon chancre; from the wasting, lassitude, and glandular swellings which usher in the coming mischief, up to the latest stage of tertiary ulceration, or syphilitic disease of the internal organs or nervous centres. The reason for thus agglomerating symptoms so widely differing from each other in character and date, the treatment of the last phase of which is supposed by so many excellent authorities to be fundamentally distinct from the first, will be seen further on.

I am not going here to re-open the old question about the superiority of simple over mercurial treatment, or of the latter over the former. After all that has been written, the dispute seems in much the same state as ever. The latest combatants who have appeared on the stage come forward, each in support of his own cause, as confident, as well supported by facts and cases, as any of his predecessors. This ever-recurring, endless conflict of opinion seems so entirely independent of the merits or demerits of the point at issue, and so entirely dependent on a cause quite beyond our control—the in-born constitution of the human mind—that I should consider it waste of time to examine arguments controverted only to start into life with renovated vigour. In a systematic work, it would doubtless be the author's duty to do so, however fruitless and unprofitable he might consider the task; but in a paper intended to be strictly practical, any endeavour to reconcile such conflicting opinions as, that the worst symptoms of syphilis are never seen except when mercury has been given, and that equally bad cases are met with in persons who have never taken a grain of mercury or used it in any form—that mercury is a poison here, as maintained by Mr Syme and many others, and the sole antidote, as taught by numbers of excellent surgeons,—would be quite out of place.

Neither will I attempt to state all the reasons which gradually led me to abandon the known methods of treatment, and to substitute that which I propose to describe here. The observations on which this system is based extend over many years of trial; during the last four or five years it has been incessantly in force, except during the periods when I was absent from work; great part of it would consist but of a long history of failures and disasters, of slight successes and frequent disappointments, which, though important enough perhaps at the time of trial, would be equally wearisome to recount and to read. I think, therefore, that it will be better to restrict myself to stating the conclusions arrived at.

The first is, that beyond all question, a great many persons get well without taking any mercury at all. So far, I think, the opponents of this drug are right. Very possibly these persons would have been cured more quickly and effectually if mercury had been given, but the fact that they get well without it—so far, at any rate, as visible symptoms are concerned—cannot be doubted. Besides, I entirely question the utility of mercury here. The patients of whom I speak are usually careless and refractory, often dissipated. They have so far done nothing for their complaint, and, most probably, would do nothing for it in the only way likely to be of service. For a short time, perhaps, impelled by the fear or suffering which some new and threatening symptom has brought on, they may conform with tolerable regularity to the rules laid down by the surgeon; but with the first feeling of relief these good resolutions vanish, and they return to their old courses as surely as the comet returns towards the sun: sometimes, indeed, recovering by the aid of a good constitution, fresh air, and high feeding; oftener, perhaps, breaking down in the long-run from the combined effects of syphilis and debauchery; or, perhaps, disappearing from our view, so that we cannot trace their course for good or for evil.

To give mercury for such a case means simply to add to the mischief, to damage the reputation of the drug and of the practitioner who prescribes it. There is scarcely any fact better established in medicine than that mercury, to be of any service, must be taken systematically; and that is one of the last things that a patient of this class is likely to do. The surgeon may as well suffer him to go his own road; for go it he will, whether he be preached to or not. We may put down in the same category with these persons all those who have a great horror of mercury, who believe they cannot take it, who make a point of controverting every rule of treatment, or who cannot be depended on as respects their habits. They may, one and all, be eliminated from the class of cases to be treated by means of mercury. The surgeon who tries to use it here will find that it is a two-edged sword, with one edge a great deal sharper than the other; the patient will blame the mercury for everything that goes amiss, and he will reap nothing but vexation for his pains.

Again, there are many cases which do extremely well on iodide of potassium. On this head there is ample evidence, and I have observed cases which might satisfy the most incredulous. Not merely do the symptoms disappear under the gentle operation of this medicine—not merely does secondary syphilis, thus treated, vanish without bringing in its wake the dreaded tertiary symptoms—but years afterwards I have examined such patients, and failed to find any trace of a relapse, or any prospect that one would occur.

If at the outset we could unfailingly diagnose these cases, I do not know that it would be necessary to treat them with mercury; probably it would be quite as well not to give it at all. I see no evidence that mercurial treatment is here in any way superior to that of iodide of potassium; it may be equal to it, but that is a different matter. The mercury can only cure after all, and that the iodide seems to do. But it is only too certain that we cannot at the beginning isolate these cases from those not amenable to the potassium, and that a very large proportion of the cases treated with the latter salt run anything but a favourable course. When this is the case, too, no increase of the dose, no degree of perseverance in the use of the remedy, however valuable such steps may be in the tertiary stage, will avail to remove secondary disease refractory to moderate quantities. I do not understand how any one can maintain that it always yields here to the iodide. My experience runs directly counter to such a statement. It might, indeed, be said that cases which do not disappear under the action of the potassium, resist mercury quite as obstinately. With a certain reservation, there is a good deal of truth in this view, and it is the worst of these cases that so often try the surgeon's resources when treating them with mercury.

I will now suppose that the process of elimination has been pursued far enough, and that we have simply to deal with the cases which remain—that is to say, with all those which we are not warranted in considering as likely to yield to time, to simple treatment, or to iodide of potassium. I suppose I am only endorsing the common-sense view of the profession, when I say that there remains for these no remedy but mercury. The reader is to remember that I am speaking here strictly of cases which hold out no promise of getting well under any other system, where the expectant method has been tried and found utterly wanting. Whether the surgeon will err on what he thinks the side of safety, and decide to look upon all cases of syphilis as, *ab initio*, fraught with mischief, is a question I reserve for consideration further on. Paracelsus said, No man knew the amount of vice there might be in an ass; and there can be no doubt that many excellent surgeons have, in the course of their practice, learned to look upon syphilis from much the same point of view, and to distrust it as much in the mildest form as in the most severe.

What the surgeon can do with these cases unless he gives mercury, I am quite at a loss to know. I therefore refrain from entering upon any fruitless discussion, and proceed upon the assumption that he has decided to try the great antidote, about which I am desirous of making some preliminary remarks, for which I ask the reader's particular attention, as they run quite counter to the views held by some eminent observers.

The first remark is, that if mercury is really to be used for the cure of these cases, it must be carried to the extent of producing a thorough effect upon the system. I do not believe a worse principle than that of simply giving mercury till the present symptoms have disappeared—of stopping short on the first appearance of any slight sign of soreness of the gums—was ever laid down. It is no wonder that some of those who treat syphilis by this "extinction" plan, should find it often recur, and, indeed, come to look upon syphilis as nearly, if not quite, a hopeless affair. No system that I have seen put in force seems better calculated to make the disease incurable, and M. Simon had M. Ricord fairly on the hip when he taxed him with the shortcomings of his treatment. In my opinion, the disease must be thoroughly subdued before any attempt is made even to reduce the amount of mercury, much less to leave it off altogether. Any half measures here are only likely to be as mischievous as semi-extirpation in scirrhus. "The back" of the disease "must be" thoroughly "broken," or it will be better for both patient and surgeon that mercury should not be meddled with.

Supposing, then, that the surgeon has decided to try mercury, the next great question is, What mode of using it should be resorted to? For my own part, I have no hesitation in saying at once that it should be employed externally—at any rate, so far as the chief effect to be expected from it is concerned. As an adjuvant to the external use of mercury, I not only never hesitate to give this drug by the mouth, but, on the contrary, invariably recommend it; but nothing would induce me to trust to it as the sheet-anchor. The most careful and judicious administration of it in this way is only too apt to be followed by most serious results, by irreparable mischief. Just enough blue pill to make the gums slightly sore, will, in some constitutions, cause the uvula to slough right off, or a piece of the jaw to exfoliate—will bring on alarming and persistent bleeding from the bowels, or an amount of prostration which compels the practitioner to give up the remedy at the very time when it is most necessary to go on with it. Granted that such accidents occur but rarely when there is a reasonable amount of prudence on both sides, it must still be admitted that they do happen at times. There are men who deny that anything of the kind ever occurs in their practice; the answer is, either that they cannot have had much practice in syphilis, or that they have not followed the cases up.

The opponents of mercury say that the worst results of syphilis are only seen when mercury has been given; and there can be little doubt that the effects of mercury given by the mouth are calculated to lend effect to the statement. Unquestionably the statement itself is exaggerated. There is ample evidence that symptoms of the worst class, especially in the form of phagedæna and sloughing, are seen when no mercury has been taken; but they are exceptions, and, as a rule, I believe the charge here holds good, and most so of all when the drug has been taken by the mouth. In my own practice, all the worst eases that I have seen were instances of this. Many of them would, no doubt, have been very bad under any treatment; they would, perhaps, have run much the same course had mercury been used externally, or possibly had it not been given at all; the use of the drug may have been only one of many causes contributing to the exacerbation of the disease; but the conclusion that its use lent a fearful impetus to the mischief seems irresistible.

Of the two remaining modes, that by friction and that by vapour-bath, I have only to say, with respect to the former, that however well suited it may be thought to a Lock Hospital, or a desperate case in private life, the recommendation to use it generally in practice must ever prove inoperative, for the simple reason that patients will, as a rule, reject it. Now and then, a man who has nothing to do may be found willing to go through a course of friction; or a person afflicted with syphilophobia may hail the proposal to do so with delight; but nineteen times out of twenty the patient will have nothing to say to it. He knows, when the matter is explained to him, that such a proceeding brings with it dirt, confinement, exposure, and discomfort, and he will rather risk the chances of a milder system. The surgeon knows that behind this lurk still more serious evils—eczema, erethism, intractable salivation—and consequently does not often exhibit any violent enthusiasm for a plan which may bring all these symptoms in its train, and may not bring relief.

Yet if it is to do good, these evils must be faced. Using inunction so gently as to steer wide of all chance of acting on the mouth, is, in my judgment, worse than useless. The present symptoms may be removed, but the disposition to still more serious outbreaks is rather exasperated than alleviated, and I fancy there are few men of experience who, when they get the chance of taking the patient in hand for a radical cure, would not much rather have a case where no mercury had been used, than one where it had been tampered with in this way.

We have now to deal with the vapour-bath, so far the cleanliest and least objectionable of all the modes of administering mercury. Its curative power is perhaps as great as that of inunction. There may be instances where a sharp salivation set up by the latter will do good where the bath has failed. Such cases have been related, but I am disposed to consider them as very rare, and to think that the chances of cure by the bath and by friction are pretty nearly balanced.

I do not wish to say one word in depreciation of Mr Lee's labours; on the contrary, I hold them in too high estimation for that; but I think their value would be greatly enhanced if they were made to comprise an accurate estimate of the relative proportion of failures to successes; and if we could gather, from reading the accounts, a clue to the cases in which vapour is likely to effect a radical cure, as distinguished from those in which it will only relieve the present symptoms, I think it is quite time that this question was stirred up. Mr Lee has done so much that is likely to survive all cavil, that he can well afford to have the subject cross-examined; and when this is done, I think it will result in the conclusion that, though the lamp-bath is the greatest improvement of modern days, yet that it still leaves much to be done. It does not get over the objections made to all systems of administering mercury. It sometimes fails; formidable symptoms, showing that it is not mastering the hold which syphilis has got upon the constitution—such as single, and even double, iritis—will appear when the patient is thoroughly under the influence of the bath; and when both these contingencies are averted, and all present symptoms disappear without any untoward occurrence, it still leaves the disposition to relapse untouched. The tendency to return on the first occasion will often resist the most persevering use of calomel vapour. I have pushed the baths to as great a pitch as the constitution seemed capable of bearing. I have continued them up to within a week of marriage, and yet have seen the first child born decidedly syphilitic.

When Mr Lee first announced the very decided success which had followed his trials with the vapour of calomel and water, I not only tried his plan extensively, but had a bath fitted up at the hospital, where I could study the effects of these remedies under my own eyes and on a large scale. But, however gently I used the vapour, or however far I pushed it, one result invariably cropped up. There was a certain percentage of failures. I then tried the addition of a small quantity of mercury, and afterwards of small doses of iodide of potassium, but with no better success. Chance led me to make trial at last of the Zittmann decoction, and with such surprising results, that I have since gradually extended the employment of it to every case and form of syphilis; combining it, however, with the iodide of potassium and mercury, and the employment of the vapour-bath, in the following manner:—

The patient is first of all put through a course of iodide of potassium and perchloride of mercury. By combining these in the same mixture, red iodide of mercury is given in a form of extreme diffusion and freshly made, conditions which enable the stomach to bear it so well that I have never yet known the mixture disagree. I would most strongly advise that, at the outset, the dose should be very small, not more than two or three grains of the potassium, and from the thirtieth up to the twentieth of a grain of the perchloride. Nothing can militate more effectually against the success of the treatment than to risk setting up irritation by giving the remedies too freely at first, or even by raising the strength of them too rapidly at any time. The object in view is effectually defeated so soon as ever symptoms of iodic poisoning begin. There is no choice but to entirely abandon the medicine for some days, perhaps weeks, but certainly until the symptoms have quite abated. At the same time, every precaution should be taken to prevent such a result arising from the doses really necessary to produce an effect upon the disease, and in practice I believe nothing answers better than to combine them with bitters and aromatics. The choice of these may be safely left to the practitioner; it is not, perhaps, of much moment, so long as the medicine is made to sit well on the stomach, and rendered palatable, but it is of great importance that those two objects should be effected, as otherwise great difficulty may be experienced in continuing a medicine which nauseates the patient at the time of taking it, and disagrees with him afterwards. I generally use the tincture of chirata with syrup of orange-peel or ginger, and cardamoms, as a vehicle; but my readers will be as well versed in such matters as I am.

At one time I conjoined those medicines with sarsaparilla; it is not likely now that I shall ever commit such a mistake again. I have used pretty well every preparation of sarsaparilla I could hear of, and long ago came to the conclusion that it was waste of time and money to employ it, except in doses too bulky and nauseous for general use; that is to say, in large quantities of the warm decoction. I have frequently prescribed the concentrated decoction in doses equivalent to twelve or eighteen ounces of the ordinary decoction, daily, for a long time together, without the patient deriving the least benefit from it.

But all precautions that I have seen put in force, for the purpose of enabling the stomach to bear the potassium and mercury, fail more or less frequently unless aperients are combined with them, and the patient is restricted to a proper diet. As to the aperient, I believe it is essential that it should consist of two chief ingredients—a pill to be taken overnight, and a draught for morning use. I have repeatedly tried both separately, and have failed quite often enough with both to deter me from any repetition of the experiment. The pill may consist of colocynth, blue pill, and hyoscyamus; or a mixture of rhubarb, soap, and jalap; a sedative or aromatic, sufficiently potent to obviate griping, is an essential feature in its composition. Naturally enough, I do not mean to say that the choice of the practitioner is to be restricted to these ingredients; he will have to select according to the constitution of the patient, and the present state of that patient's health.

For the morning draught, I would recommend salts and senna; but under that name I understand something very different from the compound usually taken, or the black draught of the Pharmacopœia; both of which, however well adapted to the ordinary emergencies of practice, are unsuitable here, the quautity of sulphate of magnesia being far too large. A small black draught usually contains quite two drachms, and when the medicine is made at home, much more is generally taken, causing an unnecessary amount of griping, without any equivalent good. About a drachm is generally quite sufficient, and many persons, especially women, do not require so much. On the other hand, the quantity of senna must be increased. It is not easy to speak with exactitude, some people being so much more easily affected by this drug than others, but generally quite a drachm is required, often more. The taste, when objected to, is easily covered by the admixture of liquorice, which also serves to facilitate the operation of the medicine. To the addition of the tincture of cardamoms I see no objection, but it is not required here as a carminative, because there is little, if any, griping set up by the medicine.

The draught should always be freshly made, and be taken in as dilute a form as possible. I advise the patient, when it is practicable, to make the medicine at home each time that it is required. The proper amount of salts, senna, and liquorice, made up in a packet, is kept ready. When the draught is wanted this is placed in a jug, a breakfast-cupful of boiling water is poured upon it, the mixture is briskly stirred to dissolve the sulphate, and as far as possible the liquorice; it is then closely covered up, and next morning strained off and drunk. The more dilute the form in which it is taken, the more certainly and gently does it act. If the patient object to diluting it before swallowing, he can do so after, by drinking half a tumbler of cold water; or, what is better, a large cup of hot weak tea so soon as he has sufficiently recovered from the fatigue of getting the draught down. As for any substitutes in the shape of tincture of senna, syrup of senna, etc., I have simply to remark that I never yet saw one of them do the least good, and that the greater the divergence in the direction of concentration and making things agreeable to the palate, the more useless does the medicine b·come.

I have often been asked by patients if they might substitute some mineral water, patent preparation of magnesia, etc., for the draught. There can be no harm in granting the request, as the patient is generally the first to find out that there has been a mistake. The moral effect of the victory gained by conquering man's natural repugnance to the unpalatable flavour of mineral waters, combined with that of superstitious faith in anything that issues from a mineral spring, may have some sort of influence; and some preparations of magnesia may take the place of the senna mixture for a brief time without any perceptible mischief; but I think that he who carefully weighs the facts of the case will incline to my belief in the superiority of the latter.

The pill and draught should be taken once or twice a week, as a somewhat free action of the bowels serves not only to favour toleration of the iodide, but to improve the health. Some persons are afraid that the continued use of such medicines must prove injurious. I believe the impression to be quite unfounded. There may be at the outset some depression after a brisk aperient, but the reaction which follows is generally attended by a feeling of relief, of greater fitness for work, mental or bodily, and better spirits; signs not at all likely to attend a prejudicial action of the medicine.

So soon as ever these symptoms are observed, the dose of the iodide and perchloride may be raised at the discretion of the practitioner. I seldom, in my own practice, go beyond five grains of the former, and an eighth of a grain of the latter, two or three times a day, and always stop short of setting up much irritation. The combined treatment is continued for four or five weeks prior to beginning with the mercurial bath, to be presently described, and, if possible, during the whole time it is employed.

Directly the dose of the iodide is increased the patient may begin to take a simple vapour-bath once or twice a week, and under any circumstances a course of these should precede the use of the medicated bath. Some of my readers may consider this precaution superfluous, but they have only to watch the progress of a few cases to satisfy themselves that the disappearance of any eruption is often directly accelerated by the vapour-bath. One author, indeed, who has tried both plans, goes so far as to affirm that the watery vapour is really the active agent in the calomel bath, and that it is the mercury which is superfluous. At any rate, the simple vapour-bath possesses one valuable property which, I think, few observing persons will refuse to accord to it—that of materially improving the condition of the skin itself, and thus very probably facilitating to some slight extent the removal of the syphilis. At one time the obstacles in the way of getting vapour-baths were so great, that, except in the case of a few persons who happened to be living near an establishment of this kind, the recommendation to employ them was inoperative; but now that a perfectly safe and portable vapour-bath can be procured for a small outlay, the difficulty no longer exists.

Having taken the simple vapour-bath for three or four weeks, the addition of mercurial vapour may be safely begun with. The quantity used at first should be very small—not more than five or ten grains of calomel for a bath—and should, I think, rarely exceed twenty grains. Some practitioners employ as much as sixty grains, but the action of this quantity is far too violent, and I have seen very painful and disagreeable lichen—threatening to run into eczema—result from it. Where the patient can conveniently resort to a bath properly fitted up for using mercury, this is, perhaps, the least onerous way of taking it. Mr Lee's bath is highly useful, and very reasonable in price. A decided improvement is, however, to substitute the flannelled mackintosh, made by Messrs Walters and Co., for the blanket. It confines the vapour better, and averts the dirty marks caused, when mackintosh alone is used, by a deposit of black sulphuret of mercury, due, I suppose, to decomposition of the calomel by the sulphur in the waterproof; or the calomel can be volatilized by strewing it on a small round plate of tin placed on the bearing ring of the portable vapour-bath lamp. In that case I advise the patient first to take a thoroughly hot vapour-bath for ten minutes or so, then to rise, dry himself, place the calomel over the re-lighted lamp, and cover himself well with the flannelled mackintosh. He thus preserves himself thoroughly warm, with a moist skin, without excessive perspiration, and gets a bath without serious fatigue or waste of time.

The bath is taken at first twice a week for a fortnight, then three times a week for another fortnight, and after that every night till, in the judgment of the practitioner, the disease seems to be effectually stamped out,—that is to say, till all manifest signs of persistence or recurrence have disappeared, and clear indications of the action of the mercury have been present for some little time. I do not find it often necessary to order in general more than about eighteen or twenty baths for this purpose. Doubt has been, as I said, cast upon the action of the mercury here, on the ground that pounds of calomel ointment may be rubbed in without producing any salivation, that the calomel is deposited on the skin in a crystalline form quite unsuited for absorption, and that the eruptions of syphilis will die out under the influence of mere watery vapour. Upon hearing this I determined to make some special investigations, and have been led to conclude that the objection is unfounded, and that the mercury, thus given, has an unmistakable action on the system, even when only applied to the skin and not inhaled at all. I also questioned the bath attendant, a very intelligent and observing woman, who has been giving baths to my patients for years, and who has had excellent opportunities of watching the action of both kinds of vapour on the skin. Her testimony was unequivocally to the effect that the mercury was more powerful than the water, and that she had repeatedly seen a "rash," refractory to simple vapour, begin to disperse as soon as ever calomel was resorted to. This statement refers exclusively to the action of these remedies on the skin alone; for though always I direct the patient to inhale, I seldom do so till he has taken half-a-dozen baths.

The baths being taken, the patient is, after an interval of one clear day, put through a course of Zittmann's decoction, lasting for eight days. The reader may think this the most extraordinary part of the whole proceeding, and I admit that it looks so; in fact, it seems rather outrageous to subject a patient to eight days' purging for secondary disease; yet I always do so where I can obtain his consent. If he will not consent, I wash my hands of all responsibility, as pretty ample observation has convinced me, that while the plan in its entirety is not infallible, there is no safety for the patient if it be left incomplete. As I always have syphilitic patients under my care, I have been able to watch the influence of this part of the treatment quite fully enough to form an opinion by this time; and after more than four years of almost unbroken observation, I feel that the observations justify me in speaking of the purging as essential.

Some time ago, after I had satisfied myself on this point, I began a series of experiments with the view of ascertaining to what features in its composition the value of the decoction is due, and have every reason to believe that the senna is the sole essential ingredient, and that it owes its value to being given highly diluted and in large bulk, and accompanied by great mixture of liquorice and aromatics. Consequently, I have one after another weeded out many of the ingredients. First, the sarsaparilla may be withdrawn, unless the great faith in its virtues outweigh the objections that it is useless and costly. The antimony and mercury are, I think, entirely useless. There is no necessity for nausea, and any power of setting up perspiration possessed by the former is left quite in the background by the action of the vapour-bath; the mercury is, I submit, better given separately, if required at all, which is rather doubtful. The carminatives may be reduced to one or two, such as cinnamon or cardamoms, or both, and the alum and sugar can be discarded; at least, I observed no diminution in the efficacy of the decoction from omitting these. Dividing the process into the two stages, expressed by the cold and warm decoction, looks rather like some astrological ceremony, or the preparation for going through a magical performance, and may be abolished at once; neither is it in the least degree necessary for the patient to drink three quarts a day; reducing the whole bulk to a pint daily is much more rational, and even this proves far too irksome to most persons, so that for some time past I have been steadily reducing the bulk of the decoction. At the same time, whatever is deducted in this way should be made up in another, and I find this easily effected by directing the patient to drink a cup of tea after the first half of the dose taken before breakfast, and a like quantity of beef-tea, chicken broth, or veal broth after the second half, which should follow the breakfast.

In this way the essential features of the Zittmann treatment are preserved, while it is more adapted to the exigencies of modern life. So many appliances are now to be found for warming fluids and procuring such things as beef-tea, hot, at any time requisite, that a bachelor in his chambers can carry out all that is really requisite quite as well as a patient in a hospital. Free purging there must be. I have seen very unsatisfactory results too often, when modifying the treatment to suit the emergencies of the patient's occupation, to trust again to anything short of this. An action which produces less than three, four, or five stools daily, I have learned by experience to regard as thoroughly insufficient; and when the decoction does not effect quite this, I never hesitate to prescribe an aperient pill every two or three nights. The decoction should always be taken warm; and when the patient can remain in bed during the early part of the day, and confine himself to the house in the evening, especially during cold and wet weather, I think he undoubtedly recovers more quickly and thoroughly.

After the course of the decoction has been gone through, dilute nitro- or nitro-hydrochloric acid, or dilute phosphoric acid, in some bitter tincture or infusion, should be given if the patient suffers from loss of appetite and debility. I suppose all belief in the anti-venereal properties of such medicines has long faded away, even if it were ever entertained by sober-minded persons since the days of Helenus Scott; but I think there can be no doubt that they materially improve the health; and unless the patient is too tired of medicine to take anything more,—which is one of the most probable things in the world,—they may be prescribed with the most confident prospect of doing good.

What I have now to add will perhaps awaken more censure and opposition to the treatment than anything else, and that is, that the patient must go through another course of the same kind, but a very short one, at the end of three months, and again at the end of six months after that. This may appear intolerable to some persens; if so, I am sorry for it, but the truth must be stated. The nature of the disease is to return, and the only safe plan is to anticipate the relapse. I do not, however, generally find it requisite to order more than a very short course; three or four weeks of the medicine, raising the dose much more rapidly than at first, half-a-dozen to a dozen baths taken every night, and a four or five days' use of the purgatives, suffice, in most cases, to do all that is requisite.

But all directions as to remedies, external and internal, the modes and times of using them, are liable to be defeated, unless the patient will observe a strictly regulated diet, and conform to the dictates of prudence. The diet should be good, without being heating or over high. I do not know whether it is necessary to define what this really means; but as it may be thought rather vague, it will perhaps be better to say, that the food should include but a very moderate amount of meat, and that such things as pork, goose, strong heating soups like mulligatawney, curry, shell-fish to any great extent, too much salmon, etc., should be excluded. Plenty of good milk, good bread, and pudding of tapioca, rice, and Chapman's flour, and mutton broth, are most desirable ingredients. Beer of all kinds, spirits, unless it be occasionally a small quantity of pure Hollands, and all strong brandied wines, should be banished at once. Sound, thin, red wine, such as good claret, burgundy, Carlowitz, and Australian red hermitage, is, I think, by far the most suitable. By conforming to the dictates of prudence, I mean that the patient should sedulously abstain from all unnecessary exposure to cold and rain, from late hours, and excesses of every kind.

This comprises all that is necessary to form the basis of treatment. Of course there are, especially in the tertiary stage, many complications which require additional means, particularly when we only see the patient after mercury has been used, such as blistering in nodes, cauterizing in sore throat, evulsion of the nails for onychia, the removal of dead bone, etc. These are matters of practice which, if discussed in detail, would carry me far beyond all reasonable limits, and I therefore forbear to enter upon them. I must equally refrain from going into the statistics of cure and failure, and content myself with saying that I have seen no system succeed nearly so well, that I have not had so far a single instance of failure or relapse after the full course where I have treated the case from the beginning, and but a small percentage of bad results in cases previously uncured by mercury; and that is equally applicable to any stage of the disease, and, so far as I can judge, any severity of symptoms.

The reader will perhaps ask if I propose to recommend this method in every case of constitutional syphilis; to which I reply, that I most certainly do, unless the patient consents quite unconditionally to take upon himself the responsibility of failure from doing otherwise. The surgeon's task is to exorcise the demon of syphilis; and, as concerns the majority of cases, I think that is best done by early and energetic treatment. Of the two errors, it is better to err on the side of safety, and safety is only to be found by looking on all cases as dangerous. Granted that a few persons throw off the disease by the inherent strength of their constitutions; they are, after all, exceptions; and any system of treatment, founded on exceptions, is as unsteady as a pyramid standing on its apex.

He may also ask if I can explain how the treatment acts; a question which may be met at once by saying that I know nothing about it. Possibly the wonderfully elastic properties of the humoral doctrine will admit of its being adapted to the case, and there is always the elimination of the virus and the emunctory property of the mercury to fall back upon. True, the humoral doctrine wants a foundation to stand upon, but that shrewd observer, Sir Benjamin Brodie, tells us that this does not interfere with the vitality of a hypothesis, which will continue to exist long after the slender foundation on which it originally rested has melted away. This may be thought improper levity, but really I do not see how the doctrine is to be met in any other manner. Reasoning and experiment are alike lost here. A chancre has been cut out, the blood from the spot unsuccessfully inoculated, and yet the chancrous action has reappeared. There is no evidence that the contamination of tbe blood is lessened by copiousness of eruption, but plenty of evidence that there is no visible connexion between the severity of secondary disease and the state of the circulating fluid. The supposed elimination of the virus from the blood goes on long after there is any virus to be eliminated; for we see it in its most formidable and destructive shape in tertiary syphilis, when neither the blood can be inoculated or auto-inoculated, nor can the disease be conveyed by contact, connexion, or descent. Yet with these facts before them, with the full knowledge that they cannot demonstrate the first stage in the hypothesis which they use so freely, and that to verify what they so unhesitatingly advance would require powers of sight and comprehension not possessed by the human race, there are men who constantly speak of elimination and blood-poisoning as though they were speaking of something established beyond doubt.

ARTICLE III.—*On Two Cases of Anchylosis of the Knee-Joint treated by Osteotomy.* By RICHARD VOLKMANN. Translated from the "Berliner Klinische Wochenschrift," 14th December 1874.

INTRODUCTORY NOTE BY MR LISTER.

THE following paper by Professor Volkmann of Halle seems to me well worthy of the notice of British surgeons, not only on account of the novelty and interest of the method of treatment which it describes, but also from the example which it presents of a surgeon, deservedly occupying a very high position in the profession in Germany, who, appreciating the paramount importance of the antiseptic principle, carries it out with an intelligent care such as can alone ensure success. The passage which I have italicized breathes the true spirit of earnest scientific surgery, and is as remarkable for the modesty as for the confidence of its tone.

Professor Volkmann's confidence, moreover, is amply sustained by his facts. The manner in which the wounds healed in the cases detailed in this paper, the blood-clot between their gaping lips becoming organized, and the epidermic pellicle forming beneath an outer layer of the coagulum, so that cicatrization took place without any suppuration, and without even the occurrence of granulation, is clear proof that he has made himself thoroughly master of the antiseptic method; and the statistical evidence which he adduces of the success of that method in his hands, including the entire banishment of pyæmia from an old and overcrowded hospital, under the most unfavourable hygienic conditions in other repects, will I trust receive the serious attention which it deserves.

The two cases here reported, in which osteotomy was performed on account of anchylosis of the knee-joint, may perhaps be considered instances of unjustifiable interference by many surgeons who are obliged to treat their patients under unfavourable hygienic conditions, and have not yet learned to appreciate the protection afforded by Lister's method of treatment.

Both patients were girls almost arrived at puberty, who had been obliged to walk with crutches for years, the knee being bent at a right angle and in a condition unfavourable for forcible extension. In one of the cases the femur only was divided just above the joint; but in the other the tibia was cut through as well, immediately below the articulation, and the angular contraction was corrected at the expense of the divided bones. Both patients have made a good recovery, and are walking about well with high-soled boots, which were rendered necessary because in both cases the affected limb had from long disuse been somewhat less developed than that of the opposite side.

The justification of operations like these undoubtedly depends entirely on the possibility of guaranteeing a successful termination. We believe that we are not assuming too much in saying that we have by degrees attained sufficient practice and experience in the antiseptic treatment of wounds, to be really able to promise such a result with certainty.

I had hoped to have been able to publish before now the communication which I made on the antiseptic treatment and Lister's mode of dressing, on the occasion of the third "Congress der deutschen Gesellschaft für Chirurgie;" but as this has unfortunately not been the case, I may perhaps be allowed to mention here a few facts for the purpose of showing how greatly the danger of some forms of injury which were formerly followed by a very high rate of mortality, is diminished by this procedure.

Since the introduction of the antiseptic method into my clinique, now exactly two years ago (at the end of November 1872), no single patient suffering from a compound fracture in which conservative treatment was attempted, has died. Amongst this number are included even those cases in which conservative treatment was only resorted to because the patients would not give their consent to amputation, and also those in which we at first underestimated the severity of the injury, and afterwards intermediate or secondary amputation had to be undertaken on account of hæmorrhage or gangrene. The number of compound fractures successfully treated without a single fatal result in our hospital, which is old and always overcrowded, and offers the most unhealthy hygienic conditions, amounts at present to 31. Amongst these were as many as 19 compound fractures of the leg, in several instances much comminuted and often complicated with most severe bruising and laceration of soft parts. There were also two compound comminuted fractures of the patella, both of which recovered with movable joints. No case of pyæmia has occurred for a year and a half, *i.e.*, since July 1873, although during this period alone about 60 major amputations have taken place.

Osteotomy has only been practised extensively during the last year, and whereas it used formerly to be restricted to a few isolated cases in the leg, its employment is now extended to the thigh. In the "Centralblatt für Chirurgie," No. 1, 1874, are published two cases of extreme contraction of the hip-joint, in which I removed a wedge-shaped piece of bone from the femur below the trochanter.

During the year 1874, I have performed osteotomy 13 times (the division of two bones in the same patient being counted as two operations), and of these 10 recovered without any suppuration at all, and the others with only the most trifling possible amount. In all the local reaction was absolutely *nil*, and in no case did the slightest redness or swelling of the soft parts take place, to say nothing of any phlegmonous inflammation. All these operations were performed with a chisel; and if, as in most cases, a simple division of the bone, and not the removal of a wedge-shaped piece, was intended, the instrument employed was extremely narrow, but at the same time rather stoutly made, with a cutting edge only two or three lines broad. To prevent the chisel from becoming jammed and sticking fast in the deeper parts of the incision into the bone, I made use of instruments of varying thickness. The superficial part was divided with the stoutest, the deeper with a thinner, and the deepest with the thinnest instrument of all, so that the cleft was slightly wedge-shaped. The posterior layer of compact tissue was never completely cut through, but was broken, so that no risk might be run of injuring the soft parts behind.

It is almost superfluous to say, that the treatment of contractions of the knee by osteotomy is only indicated in those cases where a considerable angular deformity exists, which renders walking impossible, and where at the same time forcible straightening is either impracticable or inexpedient. Such cases are, however, pretty frequent, and it would require a full discussion of the pathology of anchylosis of the knee, to exhaust all the conditions which may possibly come under this category. Wide and old osseous anchyloses will sometimes render straightening impossible, in spite of the employment of the greatest force. If there be extensive cicatricial adhesion of the epiphyses to the integuments, forcible extension may be a very dangerous undertaking. At other times the patella is firmly united to the lower aspect of the articular surface of the femur, in a position of more or less flexion, and all attempts to dislodge it are unsuccessful. As it then occupies the part destined for the head of the tibia when the leg is straight, the reduction of the latter bone to its natural, or anything approaching its natural, position is impossible, and a powerful attempt at forcible straightening could only result in dislocation of the tibia backwards.

In another class of cases a dislocation, or at all events a subluxation, has already occurred. The head of the tibia is placed behind the lower end of the femur, or perhaps still occupies a position opposite part of the articular cartilage, and is fixed here by old adhesions. The bent position of the knee allows the broad upper surface of the tibia to press against the femur, and so the flexed limb is firm and can bear the weight of the body if resting on a support. But if the knee be now forcibly straightened, and the thigh and leg are brought approximately into the same straight line, the femur and tibia come to lie one behind the other, and lose their points of mutual contact and support. The leg when thus straightened is excessively weak and powerless, and patients, as a rule, complain of prolonged pain from the stretching of the soft parts in the attempts at reduction, which pain constantly persists until the limb has returned to its old position. A recurrence of the contraction is under such circumstances very common and often altogether unavoidable.

Lastly, I will draw attention to another very interesting but hardly recognised form of alteration in the shape of the articular surfaces, which I have almost always observed in cases where contraction of the knee has commenced in early childhood, and has existed ever since. I have had several opportunities of examining this condition pathologically. If far advanced, it renders the straightening even of a partially movable joint impossible. It consists of a prolongation of the condyles of the femur, in the direction of the long axis of the bone, and it is brought about by the overgrowth of the lower part of the articular surface, because it has been released from the pressure of the head of the tibia, and from the weight of the body in walking, as a result of the permanently flexed position of the joint. A longitudinal section of the condyles of the femur assumes in consequence the shape of a long ellipse. As two of my preparations show, this elongation may reach a very extraordinary degree, and then a condition exists which is easily mistaken for a luxation of the tibia backwards, on account of the great prominence of the condyles of the femur. In such cases, however, a dislocation of this sort is by no means always present; although when it does exist it will greatly favour the development of the condition in question. I am also of opinion that where there is at first a smaller amount of flexion, it will be gradually increased by the growing projection of the condyles, while the contraction in its turn leads to further overgrowth in the femur. But if there be only a slight dislocation or none at all, the continual pressure of the head of the tibia, forced firmly as it is against the lower end of the femur by cicatricial shortening of the ligaments, and contraction of the muscles, prevents the increase of the articular end of the femur in its antero-posterior diameter, and the long egg-shaped condition of the epiphysis is considerably exaggerated by the deficiency of growth in the posterior portion, whilst the head of the tibia at the same time has its development interfered with and remains dwarfed.

The accompanying figure (not here reproduced), though rough and diagrammatic, is drawn to scale from a preparation in my possession, and will serve to illustrate this state of things, and to show that even though the joint is far from obliterated, the contraction is irreducible, because the lateral ligaments would have to be stretched to two or three times their present length, in order to allow the tibia to occupy the same position on the femur that it does when the limb is straight. When, however, the prolongation is less marked, I have seen forcible extension succeed without producing dislocation, but accompanied by distinct impaction of the projecting part.

The peculiar conditions above described were present in both the following cases treated by osteotomy.

1. Therese Tittel, a strong girl thirteen years of age, had suffered from white swelling when four years old, which, after discharging spontaneously and lasting for many years, subsided, leaving the knee flexed at a right angle. Both thigh and leg were badly developed as compared with the other side. Forcible straightening offered no prospect of a successful result; because, although a very slight movement remained in the joint, the patella was firmly anchylosed to the external condyle in the position of extreme flexion; and, further, the lower end of the femur was prolonged in the manner that has been described. And, indeed, the prolongation was so extremely great at the inner condyle that, even if straightening had been in other respects possible, it would have caused an exaggerated *genu valgum.* All attempts at forcible extension being thus out of the question, division of the shaft of the femur with a chisel was undertaken on the 8th of August 1874, the antiseptic treatment being employed. The incision in the skin was 3 c.m. long, situated 7 c.m. above the middle of the joint on the anterior and outer aspect of the limb. Simple division of the bone was performed in the manner described. No sutures were inserted, Lister's dressing was applied, and a plaster-of-Paris splint with a window and pelvic band was placed on the extended limb. No fever followed the operation, and healing took place without the slightest local inflammation or suppuration. During the first week, the wound,

which gaped slightly, was occupied by a blood clot, and then the most superficial layer of this turned a yellowish colour, and discharged traces of a serous, not purulent, orange-yellow fluid. The dressing was left on from day to day, and changed but seldom. On the 10th September, the thin superficial layer of the clot, of almost leathery consistence, separated, and underneath it healing was found to be complete. The epidermis had gradually penetrated from the cutaneous margins of the wound into the clot, separating this superficial layer like a thin sequestrum. On removing the plaster-of-Paris on the following day, the divided bone was found already consolidated. The patient was provided with a support having a sole 4 c.m. high, by help of which she soon learned to walk without a stick. The bent condition of the femur at the seat of operation is easily perceptible by the touch, but little apparent to the eye. The scar is 2·5 c.m. long, and 1·5 c.m. broad.

2. Marie Hoffman, a girl about thirteen years of age, in feeble health, had suffered from *tumor albus* of the left knee six years before, the joint remaining flexed at a right angle. A very slight amount of passive movement was possible in this case as in the last. The tibia was almost completely dislocated backwards, and at the same time there was a most unusual elongation of the condyles of the femur. The size of the limb was very considerably less than that of the right. In order to avoid excessive angular deformity, the tibia was divided below the joint, as well as the femur above it, by means of a chisel, the knee being thus rendered movable *en masse*. This was done on the 12th of August 1874. The incisions in the soft parts were each not quite 4 c.m. long, and the bones were divided 5 c.m. above and 6 c.m. below the articular surfaces. The further treatment resembled that of the last case, and the course of events was precisely the same, inasmuch as here also there was neither fever nor suppuration. On the 14th September the organization of the lower parts of the clot was complete, and on removing the dressings, the wounds were found smoothly healed over, and the bones consolidated. The girl soon learned to walk with a support, having a sole 5 c.m. high. The deformity in this patient is much more considerable than in the other case, as the whole region of the knee projects abruptly from the level of the straightened limb. The upper scar is 3 c.m. long and 1 c.m. broad, the lower is somewhat smaller.

ARTICLE IV.—*Case of Suppurative Pericarditis* (under the care of Dr G. W. Balfour). By ROBERT SAUNDBY, M.B., late Resident Physician, Royal Infirmary, Edinburgh.

SIMON FRASER, æt. 13, was admitted into the Royal Infirmary on the 21st of May last, complaining of cough and pain across his chest. He stated that he had been ill only a week, and that the attack came on suddenly. He went to bed feeling as usual at night, and next morning found himself unable to rise. Had had typhus fever five years ago, for which he was treated in the Royal Infirmary fever wards. A short time after he had to enter Chalmers Hospital for dropsy. For the last five months he had been living at home, but for two years previously he had been employed in the Industrial Brigade. During the whole of this time he enjoyed fair health. His own home was very poor; his father had recently died after a long illness, during which he became very thin and had a bad cough; his mother is still alive.

Physical Condition.—He was pale and emaciated, lying easily only on his right side. His pulse was soft, feeble, and irregular; his temperature, 102° Fahr. On inspecting the chest the bulging of the præcordial region was very apparent; there was no visible apex beat nor could any cardiac pulsation be detected by palpation; there was dulness over an area extending from the level of the diaphragm to the lower border of the second rib, and transversely in the line of the fourth rib, for a distance of seven inches, beginning two inches to the right of the sternum and passing across to the left; the heart sounds could be heard distinct and pure when he held his breath, at other times they were obscured by a loud double friction murmur most audible over the anterior portion of the left lung, and synchronous with the respiration. There was a good deal of râle anteriorly over both lungs, and posteriorly the left base was dull; the respirations were 36 in a minute. His appetite was much impaired, and his bowels were very loose. The urine, which was loaded with urates, never contained albumen at any time.

Progress of Case.—He gradually got worse, his face became puffed and his legs œdematous; his respirations still more hurried, his pulse smaller, and he frequently vomited his food; his temperature was low, never after the first twenty-four hours being over 101° Fahr., rising slightly at evening to fall in the morning. On the evening of the 10th of June, twenty days after his admission, he could no longer lie down, but sat up in bed, gasping, pallid, and pulseless. It was at once determined to tap the pericardium; a fine trochar was introduced between the fourth and fifth costal cartilages, half an inch to the left of the sternum, and thirty ounces of pus were drawn off with the aid of the aspirator. He seemed relieved by the operation, and could lie down; his pulse returned and was more regular than it had been for some time, but the area of dulness was not at all diminished. He died at 1.30 A.M., on the 11th of June.

The autopsy, made on the 12th of June, showed traces of recent pleurisy on the anterior part of the left lung, and a small quantity of serum in the left pleural cavity; the lungs were adherent to the pericardium. In the lower lobe of the right lung there was a collection of pus about the size of a hen's egg, the wall of which was adherent to the pericardium, but no opening could be discovered; no traces of tubercle were found in either lung. The sac of the pericardium was enormously distended, containing thirty-six ounces of pus; the membrane was much thickened; both visceral and parietal layers were villous, and covered with ragged lymph; the heart was bound down to the posterior wall of the sac by old-standing, completely organized adhesions. All the other organs were healthy.

Remarks.—Cases of suppurative pericarditis are rare, but cases in which the effusion is composed wholly of pure pus are, says Walshe, so rare as almost to take rank among the *mirabilia* of morbid anatomy. Bouillaud (Traité Clinique des Maladies du Cœur, tom. i.) only gives two cases in which pus was found in the pericardium; in both these cases pericarditis had not been recognised during life, and the quantity of pus was very small. The pathological records of the Royal Infirmary do not contain a single case during the last six years, so that little excuse need be offered for bringing this case before the profession.

The post-mortem examination showed that there had been long previously a severe attack of plastic pericarditis, and this we are warranted in assuming to have been contemporaneous with the dropsy which came on after the typhus fever, and for which he became an inmate of Chalmers Hospital. That the fever was typhus has been verified by reference to the books of the Royal Infirmary, but unfortunately no record of the case is to be found at Chalmers Hospital. Typhus is not generally regarded as a cause of pericarditis, but nearly allied diseases are. There was no history of rheumatism, nor is it likely that the dropsy in question was due to Bright's disease, as acute Bright does not often cause pericarditis; and if it did, it would be very hard to believe that the patient would recover with such visible traces of the pericarditis, but no structural alterations in the kidneys. The adhesions, which were very close and strong, do not appear to have interfered much with the functions of the heart, as he was considered to be an able-bodied lad when in the Industrial Brigade.[1]

The causation of the second attack of pericarditis is more obscure. Niemeyer says that suppuration may result from a protracted sero-fibrinous pericarditis, but there is no evidence to show that there had been a preceding protracted affection; on the contrary, the history tells us of a sudden and violent onset which at once prostrated the patient. The most probable explanation, therefore, appears to be that a small quantity of pus from the adjacent abscess escaped into the pericardium through a small opening, which subsequently closed up. There was no difficulty about the diagnosis; but the effusion being recognised, the propriety of tapping was considered and reserved as a last resource to avert death, from the feeling that there was something anomalous in the case; the constitutional depression being evidently greater than the amount of effusion accounted for, while the family history gave good grounds to suspect the presence of tubercle. Death was caused, without doubt, by the gradual exhaustion of the patient, the embarrassment of the heart by the accumulated fluid, and the diminution of air space in the thorax.

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[1] I have to acknowledge, with thanks, the assistance I received from Mr E. Erskine Scott, a director of the Industrial Brigade, in verifying the previous history of this case.—R. S.

ARTICLE V.—*The Morisonian Lectures, delivered before the Royal College of Physicians of Edinburgh: Session* 1874. By J. BATTY TUKE, F.R.C.P.E., F.R.S.E.

LECTURE V. (*delivered* 31*st March* 1874.)

FROM what has been said in the last two lectures, it must have been gathered that my intention was to indicate that acute brain disturbances which are evidenced by psychical alienation, are caused by modifications of the quantity or quality of the blood-supply, and that the modifications in quantity are the result of at least five physiologico-pathological processes, which serve to differentiate further between certain of the idio-encephalic and the evolutional and constitutional conditions. In the 1st, we have the primary changes in the cells, which influence secondarily the blood-supply, as in the case of continued loss of sleep and the arrestment of the recuperative quality of the cell; in the 2d, we have changes in the general nutrition of the system, implicating through the medium of the bloodvessels the health of the cell, as in the evolutional conditions, and in certain constitutional conditions; 3d, that in general anæmia the modification in the quality of the blood acts as a prime factor; 4thly, that various toxic agencies produce changes in the nervous tissue which modify the blood-supply; and, 5thly, that changes in the tissues of the vessels, as in atheroma and constitutional syphilis, may produce extensive lesions by mechanical deprivation of blood-supply. Thus, we have in the first condition the cells primarily, and in the other four secondarily affected.

Now in all recent insanities, whether they be symptomatized by acute mania or acute melancholia, the first post-mortem appearance presented to the naked eye is an injected condition of the bloodvessels, and the microscope only serves to render this condition more thoroughly demonstrable—the vessels are found full of blood, distended by it, and fully occupying the cylinders containing them: we have evidence of the existence of a condition of stasis. On the length of time taken to restore the intracranial circulation to its normal condition depends, I believe, the result of the case—recovery, or its continuance in a chronic form. In the case of recovery, a period of convalescence is necessary for the full resumption of the functions of those constituents impaired by the congestion; but if the obstruction be not soon relieved, permanent pathological changes ensue, which may be termed the secondary lesions, and which must be regarded as the causes of chronic insanity.

But how are we to account for this condition of stasis? I confess this is by no means an easy question to answer. No theory of increased mechanical pressure pumping more blood than is normal into the cerebral vessels is admissible, for the exhausted condition of the anæmic maniac precludes such an assumption, even were it warrantable on other grounds: it is to some influence on the muscular coats of the vessels themselves, impairing their controlling function, to which we must look for an explanation of the phenomenon. There are three theories which may be advanced—1st, That the degeneration and impairment of the functions of the cells of the convolutions may exert an influence on the sympathetic centre in the medulla oblongata pointed out by Ludwig, by means of the divergent or radiating fibres, which, as you will remember, are continuous with the peduncular fibres of that organ and of the spinal cord; and that dilatation of the vessel results from loss of their inhibitory function of the muscular fibres. Against this theory it must be stated, that this congestion does not extend to all the vessels of the brain, that as a rule those of the base and of those parts below the level of the horizontal limb of the fissure of Sylvius are not implicated. 2dly, It may be said that the well-known phenomenon of the increased supply of blood to morbid tracts to meet the demand called for by morbid processes offers an explanation; that the myriads of exhausted cells concomitantly depress the contiguous tissues, placing them in the position of actual disease: this appears to be a more hopeful field for speculation. And, 3dly, It is not altogether hypothetical that a change in the blood constituents occurs, the nature of which has not yet been ascertained, but a change in some degree allied to that noted in the frog's foot on irritation of the spinal cord.

It is possible that stasis may result from a combination of these influences, but at the present moment no thoroughly satisfactory explanation of it can be offered; we only can state the fact that, in all acute insanities, this condition is ever present; and I will now endeavour to point out in what manner congestion of the vessels of the brain is the factor in the production of many of the secondary lesions. But before doing so, I would repeat the observation that it is on *the superior surface of the convolutions that we find the evidence of hyperæmia and its consequences.* On removing the calvarium and dura mater we almost invariably find the arachnoid milky, opalescent, thickened in a greater or less degree, and the pia mater injected most markedly over the area of convolution exposed; for when we remove the brain from the skull the portions below the line made by the saw are comparatively healthy to all outward appearance; we find that the focus of disease is at the extreme vertex, at the point where the fissures of Rolando meet, extending backwards and forwards along the line of the longitudinal fissure, and that the manifestations of morbidity fade away laterally and are gradually lost about the level of the horizontal limb of the fissure of Sylvius, and that, except under specific conditions, the base of the brain is to all appearance as healthy in the chronic maniac as in the most mentally stable subject. Simple and self-evident as this pathological fact is, I believe I may claim to have been the first to record it—in the Sixth Annual Report of the Fife District Asylum. What is true of the naked eye appearances is true also of the microscopic; for, with the exception of the cerebellum, microscopic lesions are more pronounced in parts subjacent to thickened membranes than elsewhere. It is also well to remark here, that opacity of membranes is first traceable in the line of the vessels. With these remarks, we will pass to the factorial influence exercised by stasis of the cerebral vessels in the production of secondary lesions.

It will be remembered that the arteries and veins of the cerebral hemispheres are enclosed in comparatively rigid cylinders of nervous tissue and neuroglia; that between the brain substance and the proper coat of the vessel we have a fine hyaline membrane surrounding and encapsulating it; and that by the space, small as it is, between the vessel and hyaline membrane, are conveyed the lymph products to the lymphatics of the pia mater. We have thus a main and overflow, as it were, in one cylinder; the main, however, is elastic, and is liable to changes in its calibre to such an extent that under their influence it fills completely the retaining cylinder. *When this takes place, the overflow is occluded and rendered useless.* That the artery does in hyperæmia fully occupy the cylinder is proved by the fact, that in subjects in which congestion has been a frequent condition—*e.g.*, epilepsy and general paresis—the cerebral substance is distended and the comparatively rigid cylinder enlarged by pressure from within. But this is to be seen only in old standing cases—in recent ones the vessels appear to be, if you will excuse the paradoxical expression, fuller than they can hold. Under such circumstances, what becomes of those effete or superfluous matters and the exudates produced by hyperæmia and stasis, which should have been carried off from the surrounding tissues, and which may be presupposed to be increased in quantity to the same extent as the blood supplied? They cannot find exit by their normal passages, accordingly the brain substance and the membranes become œdematous, the pia mater becomes displaced by the serum, which, oozing through the tissues, raises it from the convolutions within and without the sac of the arachnoid, fluid becomes arrested, and the ventricles of the organ become dropsical. If relief is not soon obtained the patient dies from some complication of the general system—not unfrequently passive congestion of the lungs. If relief is found, as in the large majority of cases it is, the patient either slowly recovers mental health, or, in consequence of grave and irremediable injury done to the textures of the brain, lapses into one or other of the chronic conditions of lunacy.

It is to the study of these lesions of the brain we will now turn; and if I dwell upon them in pretty full detail, I must ask your indulgence, as one is naturally led to dilate fully on what has occupied many years of one's lifetime as a special field of investigation. That the outcome of this work is but slight and unsatisfactory compared with what might have been expected from the opening up of an almost untrodden field, there is no one more willing to concede than I; but it was necessary to break the crust of the field, and, accordingly, I now lay before you the results of the first turning over of the soil.

In the following remarks will be incorporated the results of the observations of Dr Howden of Montrose, Dr Kesteven of London, Dr Major of Wakefield, Dr Lockhart Clarke, and various Continental authors. This is mentioned now to avoid unnecessary repetition of names.

My own observations have extended over more than a hundred cases of insanity, and 25 subjects who had died without any abnormal psychical manifestations; also the brains of various animals—cats, dogs, moles, rats, monkeys, pigs, etc.—and some fishes, have been subjected to examination, for the purpose of comparison and the establishment of a criterion of health. As to the lower animals, it may be at once said, that in no single instance have any of the morbid appearances about to be described presented themselves—a uniform clearness and definition of structure has been observed.

It was my good fortune in the earlier stages of the investigation to have associated with me Professor Rutherford of King's College.[1] We together examined and reported on thirty cases,[2] and you will all recognise how much must have been lost to the inquiry when this eminent physiologist found himself compelled to withdraw from it. The method of working which we conjointly adopted has been in the main adhered to, at least as far as the preparation of the specimens is concerned, for in other respects it has been much elaborated. It is shortly as follows:—The brain having been removed not later than thirty hours after death, sooner when circumstances permitted, was first submitted to the microscope in the fresh state. Next, portions were taken from the frontal, parietal and occipital lobes, from the cerebellum and corpora striata, pons and medulla oblongata, and from tracts manifestly diseased, and immersed in chromic acid solutions until sufficiently hard for cutting into thin sections; these sections were then stained with carmine or hæmatoxylin, cleared of water and spirit by turpentine, and set up in Canada balsam. You will remember that in the first lecture it was not contended that this system of preparation showed the exact condition of the brain during life—it was only contended that the modification is so slight as to render the demonstration reliable, in so far as that the relation of constituents is not materially interfered with. That the appearances presented by the sections of diseased brains are not due in any way to the chromic acid or other chemical agents, is proved by the simple fact that the brains of animals or of healthy human subjects do not show any corresponding morbid structures; on the contrary, that thin sections prepared from them demonstrate the various constituents in regular and unvarying order, in wide contradistinction to the morbid condition.

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[1] Now Professor of the Institutes of Medicine in the University of Edinburgh.

[2] Edin. Med. Journal, Oct. 1869.

We will consider now *seriatim* the lesions, 1*st*, of the vessels; 2*d*, of the cells; 3*d*, of the nerve fibres; 4*th*, of the neuroglia; 5*th*, of the membranes; 6*th*, of the epithelium; 7*th*, changes in the amount of cerebro-spinal fluid; and, 8*th*, certain occasional morbid products. I will first, as it were, simply catalogue the various lesions, and then draw such deductions or advance such hypotheses as may appear tenable.

1*st*. *The Vessels.*—When we examine microscopically a small vessel which has been carefully dissected out from the fresh brain of a subject who has died in a state of acute insanity, it will be found to be full of blood; when carefully washed with camel's-hair brushes dipped in water and the blood removed, the first thing that attracts the eye of the observer is the presence of large amorphous masses of a dirty yellow material, and a fine granular deposit between the hyaline membrane and the outer fibrous coat in the lymphatic space.[1] The first consists of hæmatoidin—at least of what was lately called hæmatoidin, for the changes in chemical nomenclature are so frequent that one can never make sure whether a name will stand for more than a year or two. Having of late had greater opportunities of examining the brains of subjects who had died in a state of sanity than formerly, I have paid particular attention to the study of these deposits, and have found that hæmatoidin is generally to be found in small quantities on the vessels of most sane subjects, but in a manifestly less degree than in the insane; and that in the non-lunatic subject it appears to depend to some extent on age, and more especially on the nature of the disease which has been the cause of death. In fever cases, in which the insanity of delirium and coma had supervened, it is pretty well marked. But in no instance have I found it in such quantity or in such large masses as in the chronic lunatic. The whole question of hæmatoidin deposits requires careful study, as it points to dissociation of blood constituents under various morbid conditions. The second kind of deposit consists of a very fine molecular material, apparently homogeneous in structure, sometimes of a slightly pale colour, more frequently colourless, possessing feeble refracting powers, and in many ways suggesting a fatty nature. This, however, is not borne out by chemical tests, which have failed in my hands to demonstrate its actual nature. I can best compare the appearance of these particles to the spores of the favus fungus. It, like the hæmatoidin deposits, has a tendency to aggregate at the bifurcations of vessels, which is no doubt due to the sacculation of the hyaline membrane at these points. This molecular material is greater in quantity and darker in colour, in proportion to the length of time during which disease has existed. It has been found very sparsely scattered over the vessels of sane subjects who had died of fever, and in one case of pneumonia.

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[1] *Vide* Bucknill and Tuke's Psychological Medicine, plate ix. fig. 2.

When we harden and cut thin sections of the brain of a subject whose insanity had been recent and acute, we find the vessels full of blood, the corpuscles closely packed together, and the coats applied to the retaining cylinder, or, as frequently happens, a vessel is actually kinked or twisted on itself. In chronic cases, on the other hand, and more especially in epileptics and general paralytics, the transversely cut vessel is seen surrounded by a clear ring of unoccupied space, with radiating trabeculæ of connective tissue extending between the hyaline membrane and the cerebral substance. In extreme cases the cylinder has been found from four to six times the calibre of the contained vessel. From experimentation I have found that when the cerebral vessels are thoroughly congested, as after strangulation, their coats are closely applied to the cylinder; and when they are emptied of blood, as after death from hæmorrhage, that a space exists between them. It would appear from this that in health the vessel pretty nearly fills the cylinder, only allowing room for the passage of lymph products by the perivascular lymph spaces, and that the clear spaces I speak of are caused by the dilatations of the congested vessel as it produces the expansion of its surroundings. On the resumption of the function of its muscular coats it contracts to its original calibre, leaving a space, which becomes the receptacle of exudates thrown out in subsequent congestions, and so, gradually, the non-contractile brain substance is pushed further and further from the vessels. These spaces have been called perivascular canals, and have been mistaken for the perivascular lymphatic spaces, which you will remember are between the hyaline membrane and the outer fibrous coat.

In the proper coats of the arteries, various degrees of thickening are the most common lesions. As regards the fibrous coats, the inner or intima is much more frequently the seat of this condition than the outer. Hypertrophy of the muscular coat is an almost constant appearance presented in chronic cases, and the circular fibres are, as far as I can ascertain, the ones affected. It is only present in old-standing cases, and is unassociated with hypertrophy of the heart or valvular disease of that organ, or with any kidney affection. As to its causation, we are precluded from any theory founded on a propelling function of the muscular coat, as physiologists are agreed that this is not the function of the muscular fibres of arteries. Although it does not come home to our preconceived ideas that the over-exercise of inhibitory or controlling function is likely to result in hypertrophy of the tissues which exercise it, we have no reason to believe that such is not the case, and the hypertrophy of the muscular coats of the cerebral arteries in the insane affords support to such a theory. In them a constant call is made for an endeavour to maintain normality of blood-supply—that is to say, they are called on in a degree greater than in absolute health to work, and in over-work they become hypertrophied like any other muscles, whatever their function.

It was not my intention to speak of the morbid appearances presented in the brains of subjects who had died from any particular form of insanity, merely to keep to the general question; but within the last few days I have met with such important lesions in the brain of a patient, the subject of what I believe was true syphilitic insanity, that I feel compelled to depart from my proposed plan. In sections made from a subject of this disease, you will notice the arteries more or less thickened as to their muscular coats, and more especially as to their outer fibrous coats—this latter coat is to be seen surrounding the vessels in concentric rings for a very considerable extent, and appears to be cemented as it were by a viscid gummatous material in which are occasionally seen amyloid bodies.[1] The effect of this thickening is completely to occlude the vessels in many instances, and in all very materially to modify their calibre. In the case from which these preparations were made, the clinical history was perfect; on post-mortem examination, local softenings were found sufficient to account most satisfactorily for a long series of physical symptoms, and the external arteries were most markedly affected by an advanced syphilitic atheroma. The clinical history of this case will be found in the January number of the "Journal of Mental Science," and the full post-mortem observations will be published in the next issue. I will only mention here that the thickening of the arteries was best marked in the neighbourhood of softenings. This obliterative thickening of the arteries of the brain is most interesting when viewed by the light of the observations of Oedmansson and Fränkel on the condition of the arteries of the villi of the syphilitic placenta. Their description and plates indicate an identical condition.[2] These authors hold that in the placenta the thickening appears on the outer layer of the intima, and extends outwards and inwards until the calibre of the vessel is obliterated. Likewise, Byrne and Verdier consider that an obliterative thickening of the branches of the umbilical artery in the placenta is characteristic of syphilis, to which is attributed the frequent death of the fœtus.[3] The collation of these observations, so thoroughly independent, and involving such different structures as the brain and the placenta, presents a very beautiful and interesting pathological homology.

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[1] *Vide* Journal of Mental Science, October 1874.

[2] *Vide* Archiv. für Gynaekologie, Band v. Heft i.

[3] *Vide* Recherches sur l'Apoplexie Placentaire de Paris, 1868.

When speaking of normal histology, as to the prolongation inwards of the pia mater, as the hyaline wall of the lymphatic spaces which exist around the cerebral vessels, a thickened condition of it was used for purposes of demonstration. In all cases of long-standing insanity, and in old subjects who had not been insane, this membrane, which is in health very fine, perfectly hyaline, and diaphonous, becomes opaque and fibroid, presenting apparently longitudinal fibres.[1] The fibroid appearance is, however, due to puckering. It is in this condition that the hyaline membrane is so easily demonstrable. Last year a discussion raged for months in London, as to the existence of a hyaline fibroid membrane on the vessels of the pia mater of subjects who have died from the cirrhotic or gouty or contracting form of Bright's disease. Sir W. Gull and Dr Sutton have figured such an appearance, and Dr George Johnson pointed out that it can be produced by the application of certain reagents. With all the deference due to such high authorities, I may state my belief that they both are right and both are wrong. I believe that Gull and Sutton are right in asserting that they have seen a thickened membrane outside the outer fibrous coat in cases where the muscular coat was hypertrophied, but that they were wrong in supposing it to be a peculiar characteristic material—a product of a specific morbid process, arterio-capillary fibrosis. Nor can I admit that Dr G. Johnson is correct in asserting that this so-called hyaline fibroid membrane is due to the action of chemical reagents, such as glycerine, acetic acid, camphor water. It is true that these agents do bring it more perfectly into view, by occupying the space between the vessel and the membrane, while they at the same time clear up the general opacity of the superficial parts; but the hyaline membrane can be easily made out by the simple use of water alone, and in prepared specimens can be found separate and distinct from the other vascular structures.

The cerebral vessels are frequently found twisted or kinked, somewhat like what happens in overstrained cords; and the cause is probably the same in both, *i.e.*, the tendency which overstretched fibre has to kink when relieved from tension.

I have spoken of the morbid conditions of the vessels nearly in the order of their incidence. 1*st*, Simple dilatation; 2*d*, Exudate deposits; 3*d*, Opacity and thickening of their hyaline membrane; 4*th*, Dilatation of the retaining canal; and, 5*th*, Hypertrophy of the muscular coats. This is of course the sequence which might have been expected, but it has been proved by the observation of a very large number of specimens, taken from subjects whose full clinical history had been ascertained.

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[1] Bucknill and Tuke's Psychological Medicine, plate ix. fig. 3.

In rare instances, aneurismal dilatations have been found, but only in the corpora striata.[1]

Degeneration of Nerve Cells.

Considerable interest centres around the study of degenerations in the nerve cells, in consequence of the subtle and peculiar nature of their conformation. The study is fraught with very considerable difficulty, in consequence of the delicate nature of their structure, and the fineness of their processes and poles; and when this has been overcome a still greater difficulty presents itself to the pathologist, namely, that of determining whether these degenerations are of primary or of secondary incidence—that is to say, whether the degeneration which is seen upon the cell is an indication of a primary degeneration of the cell, potential in the perversion of blood-supply, or whether the appearances presented are merely secondary and sequential.

In the present state of our knowledge, this question must be left unanswered, as we have too few data whereon to base a definite opinion. Arguing from the analogies of the state of the cells of the ganglia of nerves supplying regions, the seats of localized paralysis, and the atrophy of certain cells in the spinal cord consequent on amputation of the extremities, the probability seems to be, that the lesions partake of both characters, that there are efficient and resultant abnormalities of the cells of the hemispherical ganglia. I will now briefly mention the departures from normality which have as yet been detected, and in the concluding lecture indicate the few facts which seem to have a bearing on the point at issue.

When we examine a recent specimen, taken from the convolutions of an old-standing case of insanity, it is not uncommon to find the cells represented by a fatty or oily-looking material, retaining in some degree their original shape, but which can be easily broken up by slight pressure into a confused debris; this appears to be the extreme morbid condition, for in the same specimens, cells can be found covered with a mass of fine highly refracting material, destroying the definition of the angles, and blurring as it were their contour.

The nature of this material it is difficult to determine, for the most careful application of oil tests has failed to produce any change in its condition or general appearance. When similar specimens have been submitted to hardening agents, and cut into thin sections, little change in the morbid cell is seen different from what presents itself in the recent condition.

Standing upon the table, there are several sections which illustrate very distinctly not only a molecular, but a pigmentary, condition of the cells. One of these is taken from the parietal lobe of a man who died three weeks after the incidence of violent melancholic mania, but who, for many weeks prior to the actual outbreak, had suffered from psychical influences of the most exhausting character to the nervous system. A glance will show that the large cells of the fourth and sixth layers are those which are affected, whilst those of the external layers are, to all appearance, intact. Of this case I should like to observe that much less evidence of cellular degeneration was observable in the frontal and occipital lobes than in the parietal.

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[1] Bucknill and Tuke's Psychological Medicine, plate ix. fig. 4.

Other sections before you present cells in an extreme condition of granular pigmentary, or, as it was called in the first paper written by Dr Rutherford and myself, *fuscous* degeneration; in them the cells have almost completely lost their normal appearance, and are represented by rusty-coloured masses, nucleus and nucleolus having entirely disappeared.[1]

These molecular deposits generally appear first at the base of the cell, although they are occasionally to be seen surrounding the nucleus in the first instance, but under either circumstance this body (the nucleus) is the last to be affected; indeed, it is not very rare to see a nucleus denuded of protoplasmic surrounding, and lying naked in the neuroglia.

In the subjects of senile insanity, atrophy of the cells of the outer layers of the convolution is often a well-marked condition; they are shrunk to a fourth of their natural size, are surrounded by a clear space, larger than exists in health, and their nucleolus shows as a black spot. In similar cases, and in others of old standing, in which remittent or chronic mania had been the prevailing symptom, and in which the pia mater is so closely adherent as to render it impossible to strip it off without tearing the subjacent cerebral tissue along with it, an all but total obliteration of the cellular elements has been noted; along with this is associated a general coarseness of all the constituents, the definition of the layers is destroyed, and the organ in no manner presents its normal histological appearance.

There is, in contradistinction to this atrophy, a condition in which the cells of the fourth and sixth layers are found large and dilated, puffed out as it were, so as to render them rounder and larger than in health.[2] The nucleus and nucleolus are well pronounced, and no evidence of disease further than the enlargement can be detected.

Apart from the actual condition of the individual cell, we have departures from its normal relation. You will remember that the specimens of normal brain tissue submitted to you in the first lecture, showed a most beautiful and regular fan or fountain-like arrangement. In old-standing cases of insanity, this demonstration can never be obtained; the cells are distorted as to their direction and general arrangement. The cause of this will be seen when we come to speak of the lesions of the neuroglia.

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[1] Bucknill and Tuke's Psychological Medicine, plate x. figs. 4 and 5.

[2] *Loc. cit.*, plate x. fig. 4.

ARTICLE VI.—*Glimpses at the Health Resorts of the Bay of Naples in Ancient and Modern Times.* By JOHN MACPHERSON, M.D., Inspector-General of Hospitals (retired).

IT is a somewhat trite saying, that nothing tends more to create and to cement friendships than congeniality of tastes and habits. In a similar way nothing helps more to bridge over the seeming gulf between us and those whom we call the ancients, than the discovery that in so many things they thought and felt as we do. They felt and acted very much as we do, in many matters of pleasure, recreation, or health. For the sake of amusement and instruction the Romans used to run over the world. They would visit pictures, statues, temples and ruins, and beautiful scenery too, in contradiction to what Humboldt says of their not being able to appreciate the beauty of scenery. They visited Sparta and the banks of the Eurotas and Athens, or went on to the Troad, to examine the scenes of the Iliad and the mounds said to be the graves of its heroes, just as we resort to Rome or extend our travels to Jerusalem. Roman ladies used to complain of the long absences of their lords at their pleasures and their baths, just as modern ones complain of the attractions of cafés and clubs; and in truth the baths and gymnasia, with their attendant amusements, had many points of resemblance to clubs. Doctors used very frequently to order their patients away for change of air. In those days as in the present ones, every one who could possibly manage it, endeavoured to escape from Rome before the autumn. Rome had its fevers then as now; for although it had such a water supply as no other city in the world has ever possessed, there were many marshy and undrained spots, and it had more fevers probably then than now—witness the temples (at least three) to the goddess Febris, or this anxious mother's inscription:—"Febri divinæ, Febri sanctæ, Febri magnæ, camilla amata pro filio male affecto p." For mere change of air and relaxation, all the rich Roman citizens had villas on the neighbouring hills, the sites of which, so familiar to modern travellers, it is unnecessary to enumerate. But perhaps the best notion of the usages of those days about change of air and pleasure and health resorts, baths and mineral waters, etc., may be got from a consideration of the accounts we have of the habits of Augustus and his successors. What emperors did on a large scale, lesser men did on a smaller one. Indeed it is very surprising to find how many different country-seats were possessed by some of the greater citizens, of which Cicero or Pliny the younger present perhaps the best known examples.

Augustus lived chiefly in the comparatively small house of Hortensius on the Palatine Hill. His habits were simple and his wants moderate. Although Niebuhr makes the statement, that Augustus was one of those men whose constitution does not get settled till fifty, and that after that age he enjoyed fair health, he was evidently more or less of an invalid during his whole life. After the civil wars were ended, he never rode on horseback, but used to be carried about in a litter. He was fond of lying in hot weather in the inner hall or peristyle, and being fanned to the sound of falling water. When he was much occupied with business, he used to retire from the Palatine to a detached house of his own, situated on high ground on the Esquiline, which hill had an established character for salubrity, or he would go to the villa of one of his freedmen in the suburbs. The winter of Rome did not suit him. His favourite changes of place were, in the vicinity of Rome, to Lanuvium, on the outskirts of the Alban Hills, with its wide prospect extending to the sea, to Præneste, and, above all, to Tibur, where was a villa of Mæcenas, and where he often sat in the portico of the temple of Hercules to dispense justice. He travelled always in his litter, very generally by night; and by sea, whenever he could manage it. He travelled slowly, often sleeping a night on the road, even to Tibur or to Præneste; but his great fondness was for Campania Felix, the rich Campania, and the islands of the Bay of Naples. His places were Horace's favourites too. In his last journey, when suffering from his mortal illness, and fulfilling his promise to accompany Tiberius as far as Beneventum, he took the sea route along the coast of Campania, hovered about his favourite island of Capri; joked in Greek—the court language in those days—about small matters, such as giving the name of the "kingdom of do-nothing" to a neighbouring rocky island. Thence he crossed to Puteoli, and was gratified on the way by the compliments paid to him by the crew of a vessel just arrived from Alexandria. He went on to Naples, and accompanied Tiberius a little further, and returned only to die at Nola.

Owing to delicate health, Augustus did not employ the full baths and the powerful *sudatoria* then in common use. He used instead to be frequently anointed and then exposed to heat. He suffered constantly from sciatica, which at times made him lame, and which was often relieved by the application of hot sand. As strengthening remedies, he used partial baths of sea water or of Albula water, slightly heated. He probably had not brought these waters into the Roman thermæ—this was reserved for Nero. But it was easy to get a supply of the white-sulphur water of the Albula, now as then encrusting every object immersed in it. Indeed he could never journey to Tibur, without being reminded by its odour of the neighbouring Albula, which was for many centuries in high repute. The ruins of the old thermæ are still to be found, and there is a tolerable bathing establishment. But however curious geologically the water and its lake may be, the neighbourhood is sterile and uninviting. Rome, indeed, had abundance of warm waters within quite a short distance—no fewer than four in the district between Cære, L. Bracciano, and Civita Vecchia, three of which are still in use; but it is not my wish to be detained at Rome, and I must pass on to the south; therefore nothing more shall be said of Augustus and of his health, except that he was relieved of a seemingly hopeless illness by a water cure practised by Antonius Musa; and that the success in this instance lessened for a time the popularity of the warm waters, and made it fashionable to be treated at Gabii, near Rome, or at any place where a supply of cold water was available.

The successors of Augustus all had the same fondness for the south. One need only refer to Capri, the pleasure preserve of the vicious yet able Tiberius, who died in what was formerly the villa of Lucullus, on Misenum, while waiting for a quiet sea to enable him to return to his island home, his end probably accelerated by some of his attendants. Caligula is remembered, among other things, for his new bridge of a double row of boats extending from Baiæ to the mole of Puteoli, and for the infernal cruelty with which he contrived, that many who had been invited to drive on the bridge should be precipitated into the sea. He used also to while away his time along the shores of Campania, amidst dancers and symphonies, in huge unwieldy vessels. These he had built of cedar, with begemmed poops and coloured sails. They contained a vast extent of baths and porticos and triclinia, and had even a quantity of vines and fruit trees on board. Claudius was fond of the waters of Sinuessa, the modern Mondragone, between Gaieta and Cumæ, and tried its salutary air, whether he ended his days there or not, about which there is some doubt. Nero was constantly about the shores of the Bay of Naples, whether exhibiting himself as a singer to the plaudits of the mob and of the Alexandrian strangers in that city, or planning impracticable reservoirs of mineral waters, or plotting and carrying out the murder of his mother, with a flimsy attempt to conceal his share in it. Domitian had his villa at Baiæ, famous for its tame fish. Hadrian, traveller, architect, philosopher, author, soldier, and emperor, built a palace at Baiæ, and died there in spite of its waters, suffering intense agony, from which he could not induce any of his attendants to relieve him, by putting him out of pain; and "killed," as he was perhaps among the first to say, "by the multitude of his doctors." Antoninus Pius used to visit his lands in Campania every year; and it was said in praise of M. Aurelius, that he would come up for the transaction of business with the Senate, even from Campania. Alexander Severus built a palace at Baiæ, and especially a villa and stagnum or tank for his mother Mammæa—a tank being considered in those days a great addition to every mansion, as it is now by the richer natives of India. Alexander Severus deserves special notice as a great patron of bathing. He built baths in cities where he found there were none. He completed the baths of Caracalla, built the Alexandrine ones near those of Nero, and planted trees round them. He supplied the thermæ with oil lamps, for before his time they were not lighted, and therefore were not open before daylight, or after sunset. He put a stop to the common baths for the sexes, which had been forbidden by Hadrian, but the prohibition of which had been taken off by Heliogabalus. His habit was to play at ball or some lighter game, then to be anointed; after that he used the swimming-bath (he seldom used the *calidarium*), and after that dined. Almost the only exception to this predilection of the early Roman emperors for the south, is to be found in Vespasian and Titus, father and son, both born in the Sabine country, and as fond of their native air as Horace was of it. Strange to say, they both died in the same villa at Cutiliæ, near the modern Riate. The waters of that place, now the Bagni di Paterno, were very celebrated for many centuries, though they are almost forgotten; and popular fame says that the father hastened his end by drinking too much of the cold waters, while still less certain tradition says that Titus induced his fatal illness by bathing in the waters when overheated. Two such deaths should have ruined entirely the reputation of a watering place, unless its waters had real virtues.

With some such exceptions, the great health and pleasure resorts of the Romans for at least a century before and for many centuries after the Augustan era, were along the shores of the Bay of Naples. Not including Sinuessa, with its soft climate and relaxing waters, and commencing with Linternum, they extended all round the promontory of Misenum, by Baiæ, Pozzuoli, and Naples, as far as Stabiæ, the modern Castellamare, and Sorento. The islands of Ischia and Capri must be also included, although the former was too subject to volcanic phenomena to make it always a pleasant residence, and the latter was known chiefly as the abode of Tiberius. It has been seen already what palaces the emperors had at Baiæ. All the great citizens also had some of their villas in that district. For instance, Scipio Africanus retired to his home and farm at Linternum. Marius, Pompey, and Cæsar had villas at Baiæ, for which they had selected elevated sites, with a half military eye, as Seneca remarks. Cicero's Cumæan villa was also on a height. The villa of Hortensius, near the shore, was famous for its wonderful collection of trees and of fishes, but has long been submerged. Cicero had a second, his Puteolean villa, besides his Pompeian one on the other side of the bay. On the promontory of Misenum was the luxurious residence of Lucullus, with its wide sea prospect. But it is unnecessary to pursue this subject further, or to show how constantly emperors and citizens sought relaxation or pleasure, and sometimes ended their days, in those attractive spots.

The whole country must have been densely peopled in those days—containing the ancient and important city of Cumæ, already in a state of decay; the headquarters of the Roman fleet at Misenum, the crowded watering-place of Baiæ, the port of Puteoli, the seat of extensive commerce, especially with the east; and the city of Parthenope, the new city, the Neapolis, which has gone on increasing to its present enormous population, while all the other cities have gradually dwindled away. Many of these places are now scarcely habitable, owing to their feverishness. What causes, besides diminished population and the attendant non-cultivation of land, have led to these results, it is unnecessary to inquire here; but there can be little question that in those days, although, no doubt, like all parts of Italy subject to fever at certain periods of the year, the district of Campania (giving its fevers occasionally then as now) was essentially healthy, and did not suffer from fever in autumn nearly as much as imperial Rome, or the notoriously unhealthy flat coast of Etruria. In all periods sea-bathing and the mineral waters were one great attraction of the district.

The oldest name for the baths of Baiæ was the Cumæan waters, but this was because Cumæ was the great city of the district, not that Baiæ was near it. I have wandered over the scanty ruins of Cumæ, and with almost a feeling of awe, from the venerable antiquity of its history, and the close vicinity of the seat of the Cumæan Sibyl, whose utterances were not only heard with deference by the Pagan world, but were greeted with the highest respect by early Christian writers. I had just passed Avernus, with all its associations, and recollecting that Virgil's account of the infernal regions (derived no doubt from earlier philosophy) had contributed so materially to building up the doctrine of purgatory.

I climbed the rocky hill on which once stood the famous temple of Apollo; it required no great stretch of imagination to fancy the galleys of Æneas drawn up on the strand beneath. I looked down on the one hand on the Lake of Licoli, where Scipio had spent his last days away from his ungrateful country, with the long stretch of coast ending in the headland of Gaieta, and the barren island of Pandataria almost recognisable, if the day had been clearer—a rock which had received no fewer than three disgraced imperial ladies, one of them the only daughter of Augustus, Julia, whom he was slow to give up faith in, but whom, after having once done so, he never forgave. While the broken peaks of Epomeo and the varied outline of Ischia nearly faced me, I saw on the other hand the Acherusian lake, with ruins of villas on the slope around, and just beyond it the large villa of Vatia, whose calm retreat Seneca envied, at least in a certain sense. I next drove past the Acherusian lake, now the lake of Fusaro, and through the supposed Elysian fields to the Mare Mortuum; but there are no springs and no remains of thermæ, not even where they are said to have existed near the temple of Apollo, which once occupied the arx of Cumæ; although there are abundant ruins of villas, nothing reminds one of baths or springs, till one turns round to the Bay of Baiæ. The Cumæan shores are low and flat, and must always have been at some parts marshy and unhealthy.

We are perhaps in the habit of exaggerating the beauty of the golden shores of Baiæ. After all, did not the ancients praise Baiæ most for its comforts and its luxuries? It was its statues, its columns, its pavements, its marble walls, that so struck Aristobulus, King of Judea. The shores were called *blandæ* and *amœnæ*—mild, cheerful, shining, liquid, pleasant, most pleasant. Seneca said that he could not deny that the place had certain natural gifts, although it was such a sink of voluptuousness and vice. Yet if the scenery of Baiæ be not of the highest order, it has always had a very gracious natural beauty, which has not disappeared along with its artificial aids. The sea and the headlands remain with their deep colouring; so, too, the encircling hills, although they are of no very striking height, and are somewhat barren. The old glories of Baiæ are gone; the temples, the thermæ, the swimming-baths, the gymnasia, the *hebeteria* of Nero—probably meant for the sailors of the fleet—the palaces and villas built on piles out into the sea, exist only in traces or are totally submerged. The crowds of idlers wandering by day and night along the shores, or sailing in painted boats, the gay dresses of the Roman women, the shops and bazaars, the painted pleasure-boats—all the equivalents of Rome by the seaside—are gone; and we have in their place only one or two second-rate inns, maintained chiefly by the visits of curious travellers. The Lucrine lake, formerly the secure scene of all kinds of aquatic amusements, has since the eruption of 1538 been shorn of its proportions, and had even before that date reverted to its ancient use of a breeding ground for oysters.

As the most extensive modern baths do not approach in size the greater Roman works of the same class, so the dissipation at modern watering-places cannot vie with the open, voluptuous debauchery of the Romans. Even the most luxurious and idle among us, do not devote to the bath the space of time which was allotted to it by the ancients. Bathing and being anointed was one of the occupations of the day, and all the Roman annalists give details about the personal habits of the emperors in this respect—how much time they gave to the bath, whether they used the *calidarium*, how they were anointed. Heliogabalus built new baths, bathed in them once, and then had them pulled down. Another emperor put the bath-keepers to death, because they had allowed the new thermæ to be used by some one before him. Nero was particularly connected with Baiæ. He enjoyed sea-bathing both at that place and at Puteoli; and he frequently made use of the baths of his friend Piso at Baiæ before his banquets—for the Romans thought that bathing stimulated their appetites, and frequently bathed two or three times a day with that object. Nero also often spent his quinquatrian holiday in the end of March at Baiæ.

It would be easy to fill up a *Kur*-list of illustrious visitors to the shores of the Bay of Naples. Among the earliest, we hear of Hannibal, and Scipio Africanus, and Marius. All the chief men of the Augustan age visited them, and all the writers give some notices of them, which are, unfortunately, of too general a character to be of much use to us in defining localities, or the special virtues of the different waters; they also have constant allusions to the pleasures and vices of bath life. A picture of the latter is given an age or two later by Seneca. Dion Cassius tells us that the waters were good both for pleasure and for cures. There were numerous waters, many of them hot enough for boiling food and for heating cold water. They were used as plain baths, and still more as vapour-baths, Licinius Crassus having his vapour-baths rising out of the sea. The oysters and mussels of the Lucrine lake and the murices of Baiæ were then in high repute. In the time of Antoninus, or about 379 of our era, the bathing establishments continued at their full height. Cassiodorus, two hundred years after, mentions them as the resorts of officers after the perils of war. Messüe, the Arabian physician, in 1017, enumerates four of the baths of the Bay of Naples; and towards the end of the twelfth century, Alcadinus, Sicilian physician and monk, gives us, in his quaint Latin elegiacs, an elaborate account of each separate spring. These were the days of the rise of the University of Salerno, the first medical school of modern times. There is an incredible story told of its professors, which shows at least how high the repute of the waters of Baiæ must have continued to be. It is said that, owing to their jealousy of the waters, which drew away so many of their patients, Sir Antonius Sulimela, Sir Philipus Capograssus, Sir Hector de Procita, sailed in a small vessel to Puteoli, destroyed with iron instruments the inscriptions over the baths, and on their return were all miraculously and deservedly wrecked, and drowned. A story that one could credit of medical students, certainly not of the professors of Salerno!

The places along the coast have suffered repeatedly from Huns and Saracens. The coast must also have long been subject to elevations and depressions; but the district was much frequented until the famous eruption of 1538, which led to most of it being deserted. Often though it has been quoted, the old account of the formation of Monte Nuovo will bear to be given again, as recorded by Falconi, an eye-witness at least of the later part of the eruption:—"They began to see, on the spot between the hot baths or sweating rooms and Triperguli" (where there were three buildings, one of them an hospital, containing beds for thirty patients, and another of them a church) "flames of fire, which first made their appearance at the baths, then extended towards Triperguli, and fixing in the little valley that lies between the Monte Barbaro and the hillock called del Pericolo, in a short time the fire increased to such a degree, that it burnt open the earth in that place, and threw up so great a quantity of ashes and pumice-stones, mixed with water, as covered the whole country. Going next day, I saw that the sea towards Baiæ had retired a considerable way.

Turning towards the place of the eruption, you saw mountains of smoke, part of which was very black and part very white, rise up to a great height, and in the midst of the smoke deep-coloured flames rose up at times with the noise of artillery. The eruption began on Thursday night, and went on a whole week. On Friday and Saturday nights but a little smoke appeared, so that many, taking courage, went upon the spot, and say that, with the stones and ashes thrown up, a mountain had been formed in that valley, not less than three miles in circumference, and almost as high as the Monte Barbaro, covering the Canettaria, the castle of Triperguli, all those buildings, and the greater part of the baths that were about them, extending south towards the sea, north as far as the Lake Avernus, west to the Sudatory, and going east to the foot of Monte Barbaro—so that this place has changed its form and face in such a manner as not to be known again: a thing almost incredible, that in so short a time so considerable a mountain could have been formed. On its summit there is a mouth in the form of a cup, which may be a quarter of a mile in circumference, from which there issues a constant smoke." Falconi, summing up, says, "We had thus the earthquakes, the eruption, the drying-up of the sea, the birth of springs, a quantity of dead birds and fish, and innumerable trees in all that country thrown down and covered with ashes."

The chief effects of the eruption were, besides the formation of Monte Nuovo, the filling-up of the connexion between Avernus and the Lucrine lake (thus restoring Avernus to its natural condition), and the total destruction of the baths of Triperguli. The Sibyls' cave does not appear to have been materially interfered with. Christian tradition followed the old superstition that the entrance into Hades was in this neighbourhood, and, according to it, the descent of our Lord into hell, and the rescuing its prey from Hades, was an event which took place at the bath of Triperguli, a hill in the neighbourhood having been called Mons Christianus in consequence. Indeed, there are scarcely any modern hot waters which do not bring us back to the mediæval purgatory by names such as the Inferno or Limbo. Although we do not learn that many other springs besides those of Triperguli were destroyed, yet, as the inhabitants deserted the district, all the baths suffered in the general decay. A hundred years after this we have still accounts of the existence of some baths along the coast from our countrymen Evelyn, Burnet, and Addison, as well as from Italian authors, but they were mostly in a state of neglect and ruin.

(*To be continued.*)

ARTICLE VII.—*Case of Hydrocystis Capitis, occurring in a Child after Birth.* By A. R. M'RAE, C.M., M.D., Penicuik.

(Read before the Obstetrical Society of Edinburgh, 25th March 1874.)

MRS G., aged 22, a primipara, a small-made woman, was confined on the morning of the 14th June last at 3.50 of a female child. She had been in labour twelve hours, and I found it necessary to complete the labour, under the influence of chloroform, by means of the forceps. The cord was twisted once round the neck, and, owing to the narrowness of the passages, the shoulders were detained in the birth longer than is usual. The child seemed in every way healthy. The head was somewhat elongated from the chin to the vertex, and there was a very slight abrasion of the epidermis on both temples, caused by the forceps; otherwise, the head seemed normal. These slight abrasions disappeared in two days.

During the day, a soft tumour appeared on the left side of its head, extending perpendicularly from the tip of its ear to near the parietal suture, and transversely from the occipito-parietal to the fronto-parietal suture, and raised only a few lines above the ordinary convexity of the left parietal bone. Being soft and puffy, with no undue heat, I considered it to be œdema of the subcutaneous tissue, resulting from undue pressure during birth.

On the 20th June, six days after birth, the tumour had sensibly increased in bulk, and extended from the mastoid protuberance to within half an inch of the parietal suture, and from a quarter inch of the occipito-parietal to a quarter inch of the fronto-parietal suture, and was somewhat ovoidal in shape. The swelling rose abruptly from a well-defined edge, and felt quite soft and flaccid. The skin over it was of the same colour and appearance in every way as on the other parts of the head. So also was the distribution of the hair, and the temperature was quite normal. It yielded at every point to pressure, and resiled instantly on the pressure being withdrawn, thus differing from the colourless dimple left by pressure in anasarcous conditions. Palpation at every point elicited the *contré* wave so typical of hydrocele and ascites. Pressure on the left carotid and jugular vessels did not alter its size or colour. Crying very slightly increased its tension, but did not change its colour more than that of any other part of the head. The base of the tumour was formed by the left parietal bone alone, and it was evident from the above-mentioned symptoms that fluid existed between it and the skin. It was evident also that there was no direct communication between this bag of fluid and the cavity of the cerebral membranes, for pressure did not perceptibly diminish its contents, and it was transparent like a hydrocele. From these, and there being no increased temperature, it was evident that the tumour was cystic, and contained serum. At the circumference there existed a raised and roughly defined margin of two lines in thickness and of about the same height, and the surface of the bone felt, through the skin and fluid medium, rough and uneven; but it seemed generally to be on a level with, or, perhaps, speaking more correctly, seemed concentric with, the neighbouring bones. Between the outer rim of the elevated margin and the sutures of the neighbouring bones, there appeared to be a solution of bony continuity, yet no motion or crepitus could be elicited; nor did the examination seem to give the least pain.

The child, however, was otherwise quite healthy; and although the parents were urgent for the adoption of some line of treatment, I considered it best to wait.

On 28th June the tumour still appeared to be increasing in size, and palpation gave a distinct counter wave.

On 1st July the anterior portion was more bulging than the posterior, was harder, and felt as if the external plate of the bone was separated from the internal plate. The tumour was now large enough to fill one's hand.

On 15th July the tumour seemed decreasing in size. The surface of the parietal bone felt rough.

On 25th July it was much decreased.

On 13th August it had altogether disappeared; the surface of the parietal bone was smooth, the sutures and fontanelles were normal. The tumour had existed altogether for sixty days.

For a considerable time I was somewhat puzzled to know whether any, and if any what, course of treatment I should adopt. Several plans—such as strapping, iodine, the external application of, or the internal use of, scoparium, iodide of potassium, blistering, mercurial inunctions, digitalis, or any of the acetates, tapping, and the aspirator—suggested themselves; but as the abnormal condition did not seem inimical to the life of the child, or cause even any aberration of function, while any of the above-mentioned plans might have been followed by untoward symptoms, I thought it best to await the course of nature, or the advent of any symptom that might justify interference; and while thus contemplating, the *vis medicatrix naturæ* principle came to the rescue, and the tumour disappeared, thus illustrating the fact that nature may sometimes rectify itself, and the remedies applied may get the praise; also teaching us the lesson, not to be too ready to believe in the curative virtue of drugs.

I find that few cases of this kind have been recorded, while cases of cephalæmatoma are less rare. In the *Provincial Medical and Surgical Journal* for December 1844, Sir John Fife reports a case in which he tapped the cyst, and removed thirty-two ounces of fluid, and then applied strapping. The child did very well until next day, when it died rather suddenly. Now, it is equally uncertain in this case whether the fatal result was due to the disease or to the operation; but the operation was performed, and the child died. It is obvious therefore, from these two cases, that the question of treatment or no treatment cannot be settled until a sufficient number of cases are recorded. There are many points that have to be considered before a course of treatment could be adopted with good scientific reasons, such as cause, the state of health, and the presence or absence of symptoms certainly or presumptively depending anatomically upon its situation. Undoubtedly the situation of the tumour has the most important bearing on the course of treatment; and although hydroceles may be cured by strapping, yet one would be shy of strapping a cystic tumour communicating directly with the cerebral membranes, or tapping and injecting iodine under like circumstances. But the internal remedies mentioned above may not be so objectionable.

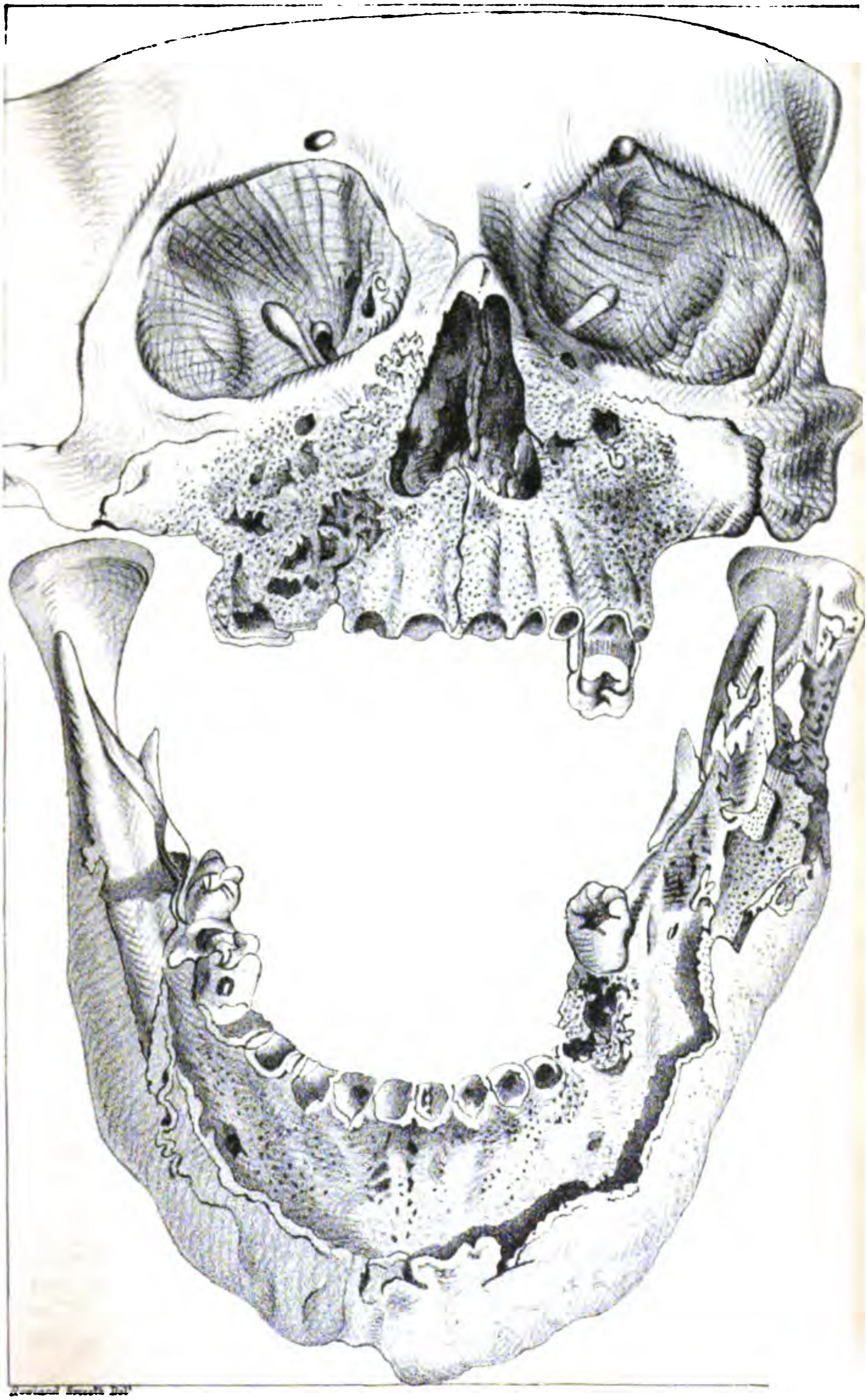
In Braithwaite's Retrospect of Medicine, vol. xii. p. 323, Mr Kesteven records a case almost similar, but he considered the case one of cephalæmatoma. The treatment was expectant, and the result a cure.

ARTICLE VIII.—*On a Case of Necrosis of the Jaws from Phosphorus.*

By J. J. CHARLES, M.A., M.D., Demonstrator of Anatomy, Queen's College, Belfast.

SINCE red or amorphous phosphorus has replaced to a great extent ordinary phosphorus in the manufacture of lucifer matches, necrosis of the jaws from this cause is of comparatively infrequent occurrence. In the month of November last a female subject was brought to the Belfast Anatomical Rooms, alleged to have died from this disease. The body was pale and exceedingly emaciated, and the internal organs small and bloodless. The small intestine was nearly empty, contracted, and of a whitish colour externally. The interior of the mouth presented a horrible appearance, and had a very offensive odour. The gums, which had receded from the alveolar processes of both jaws, were thick and swollen; and the mucous membrane of the hard palate left the anterior third of the palate processes uncovered, and formed a dense hard layer almost half an inch thick. The soft palate, as well as the mucous membrane around the fauces, was also very much thickened. There were only three teeth in each jaw, and the most of these were loose: in the upper jaw were the second bicuspid, first and last molar—all on the right side; and in the lower jaw the first and last molar on the right side and the last molar on the left. The sockets of the other teeth were unclosed, and filled with a thick dark-coloured pus. The upper jaw was affected chiefly on the left side, and the lower jaw on the right; and the necrosed parts were quite denuded, rough, and of a dark gray or brownish colour, and bathed in fœtid pus. The geniohyoid, geniohyoglossi, digastric, mylohyoid, as well as the

Dr CHARLES ON NECROSIS OF THE JAWS FROM PHOSPHORUS.



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left masseter, depressor anguli oris, and depressor labii inferioris, had formed attachments to the new bone.

The Superior Maxilla was quite dark and dead as high as the infraorbital foramina, and on the right side the alveolar process, bearing the canine and bicuspid teeth, had been broken down, and was in great part removed, leaving an irregular gap. The hard palate was unaffected, with the exception of about a quarter of an inch on each side along the alveolar process.

The Inferior Maxilla.—On the right side the body only was necrosed, and that as far back as the first molar tooth; but on the left side the disease involved the whole of this portion of the bone, except a part of the condyle and of the coronoid process. The bulk of the inner plate of the alveolar process, however, was almost healthy, but much of the comparatively healthy portions of the jaw, such as the left ramus, presented a rough surface from a deposit of inflammatory products on their exterior.

The new bone appeared only in connexion with the lower jaw.[1] It followed the outline of the bone, and formed a hollow case for the lower part of it, reaching upwards externally to the oblique line, and internally to the mylohyoid ridge; but behind the portion of jaw for the incisor teeth, it mounted as high as the margin of the bone, for an inch on each side of the middle line: it extended from nearly the last molar tooth on the right side to the tip of the coronoid process and neck of the condyle on the left side, covering almost the whole of both surfaces of the ramus. The new bone was strong, in some parts porous, and firmly joined at each end to the living portions of old bone, though loosely attached to it elsewhere. It was developed on the osseous surface of the periosteum, between it and the old bone;[2] and it presented two or three cloacæ, of different sizes, on the inside of the left angle, and near the lower border of the jaw below the situation of the left bicuspid teeth. The *spinæ mentales*, which were formed upon the new bone, were as large as usual.

History.—For most of the following particulars I am indebted to Dr Strahan, Resident Surgeon, Union Infirmary, Belfast. I. G. was admitted into the Infirmary, 8th June 1874, under the charge of Dr Brice Smyth. She was 28 years of age, married, and the mother of a large family, many members of which are still alive and in good health. For many years she was engaged in lucifer-match manufactories in Glasgow and Belfast,[3] first as "dipper" and afterwards as "packer," but declining health compelled her to abandon this employment. She was then for a short time in the Union Hospital, suffering from general debility, and for more than eighteen months she complained of pain in her jaws and much swelling of her face and gums. On admission she presented a miserably cachectic appearance, was much emaciated, and suffered severe pain in both jaws. Pulse weak and frequent, bowels regular, appetite bad, and breath of an offensive odour. Her face had a remarkable expression from the fore parts of her cheeks being greatly swollen. A hard thick ridge could be felt extending along the front of the lower jaw from one ramus to the other. The gums were considerably swollen, and the teeth slightly loosened. A few months afterwards all the teeth—first in the lower jaw and subsequently in the upper—became quite loose, and pus began to well up by the side of each. Very soon, however, her teeth caused her so much annoyance in mastication that some of them had to be extracted every day until scarcely one was left. The alveoli, however, showed no signs of closing, but remained as open as they were immediately after the teeth had been withdrawn; and pus continued to flow from them. Shortly afterwards the gums—first in the lower, and then in the upper jaw—receded from the alveolar processes, and finally left them entirely bare, the exposed bone being of a dark-gray colour. The patient was thus reduced to a most pitiable condition. She was totally unable to masticate, and lived principally on milk and arrowroot. She lingered on in great pain for nearly six weeks, and required a considerable quantity of stimulants to support her strength. She became gradually weaker, and at length found it impossible to crawl from her bed. For a day or two before her death, she was unable to move herself in bed or to articulate distinctly. But she succeeded in expressing a desire for wine, by making a slight sound and waving her hand.

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[1] Mr Thomas Smith gives a very plausible reason for this peculiarity in such cases. He states that "the lower jaw, in its structure and in the disposition of its periosteum, is comparable to a long bone, while the upper, in the same particulars, resembles a bone of the tarsus or carpus."—*St Barthol. Reports*, p. 107: 1865.

[2] Mr Smith believes that, in the case described by him, the new bone was developed, on the outside of the periosteum of the old bone, from the soft structures.—*Op. cit.*, p. 105. I fancy this must be an exceptional occurrence.

[3] I am informed that ordinary phosphorus is used in the manufactories in which the patient was engaged.

The treatment pursued was uniformly of a tonic and stimulating character, with antiseptics to allay the fœtid odour of the secretions of her mouth. At one period in the progress of the disease, it was intended to remove the lower jaw, but owing to the low state of the patient's health, this operation was considered unadvisable.

ARTICLE IX.—*Case of Salivary Calculus in the Parotid Duct.* By JOSEPH BELL, F.R.S.E., Surgeon to the Royal Infirmary.

IN January 1875, I saw, along with Dr Matthews Duncan, a young lady, æt. 23, who, for some months, at least five, had noticed a hard swelling in the left cheek, far back, nearly opposite the last molar tooth. There was not much pain, except at times, when the tumour increased in size; there was much thickening of mucous membrane, and a constant discharge of pus into the mouth. There was also considerable fœtor. The mucous membrane was red, swollen, and at one point a most malignant-looking fungating mass of granulations protruded. A gumboil was supposed to have originated the mischief. Finding that all the pus came from one point, I passed in a fine grooved probe, and succeeded in striking a hard mass at the distance of about an inch from the opening.

Next day, Dr Hardie put her under chloroform, and by slitting up Steno's duct to the distance of nearly an inch, I was able to dislodge and remove a pear-shaped salivary calculus about the size of a hazel-nut.

A very rapid recovery from all the symptoms ensued, and no deformity remains, as it was done from the inside of the mouth.

I have on several occasions seen and removed large calculi from the duct of the submaxillary glands, but believe that Steno's duct is a much less common situation for their formation.

Part Second.

REVIEWS.

The Science of Homœopathy, etc. By C. J. HEMPEL, M.D. New York: Boericke and Tafel: 1874. Pp. 177.

The Influence of Homœopathy on General Medicine since the Death of Hahnemann. By R. E. DUDGEON, M.D. London: Henry Turner and Co.: 1874. Pp. 39.

TEKEL and not ICHABOD is the word to inscribe over Hahnemann's tomb: not *Ichabod*, because no glory was ever connected with his name or theory; *Tekel*, because he and his theory have been weighed in the balance and found wanting. Of this Hempel affords most convincing proof when he says, "One of the most characteristic distinctions which separate the literal followers of Hahnemann from the progressive cultivators of the science of homœopathy, is the theory concerning doses. The former generally hold to the doctrine that anything more material than an infinitesimal dose of medicine is a violation of what they are pleased to term 'pure homœopathy.' The latter, among whom are to be found the most enlightened and liberal-minded homœopathists in this country, as well as in Europe, hold that any dose, large or small, is legitimate and scientifically correct which will effect a cure in the most thorough, safe, and expeditious manner."—(P. xv.) In casting away infinitesimals, however, homœopathists turn their backs upon history, and wilfully ignore the fact that it was the signal failure of the selections of the drug upon the principle alone, to cure *tuto, cito, et jucunde*, that led Hahnemann to develop his wondrous theories of medicinal exacerbation and the potentialization of drugs.

The supposed law involved in the dogma, "Similia similibus curantur," has been indeed more or less distinctly recognised in all ages, not only by the public, but also by poets, philosophers, and physicians. We shall quote only Homer, Plato, and Hippocrates, trusting to our readers to supply—each for themselves—the many similar passages which prove the existence of similar views in older as well as in more recent times. Homer says, *Νυν μεν δὴ μαλα παγχυ κακος κακον ἠχηλαζει· ὁς αἰει τον ὁμοϊον ἄγει θέος τον ὁμοῖον.*"[1] Plato, "*Οὐκοῦν και τοῖς των σοφωτατων συγγραμμασιν ἐντετυχηκας, ταῦτα αὐτὰ ἄλεγουσιν, ὅτι τω ὁμοίῳ ἀναγκη ἀεὶ φιλον εἰναι; εἰσί δε που οὑτοι οἱ περι φύσεώς· τε καὶ του ὅλου διαλεγόμενοι καὶ γραφοντες.*"[2] While Hippocrates, apart from many similar statements in his "Aphorisms" and in his other writings, says expressly in his book "De Morbo Sacro," "*ἀκεστα τε τά πλεῖστα ἐστιν αὐτοῖσι αφ' ὅτων και γίνεται. ἕτερον γάρ ἑτέρῳ τροφή ἐστι, τῶδὲ κακωσις.*"[3] But it was left for Hahnemann to elevate this dogma to the dignity of an exclusive principle all-sufficient for the selection of a remedy in all and every disease. As his own writings sufficiently explain, he very soon found this so-called principle insufficient for what he desired. With the most carefully selected remedy, diseases many a time were not cured, but got worse. To explain this he propounded the apparently reasonable doctrine, that if a drug which naturally produced any given disease—and therefore, from his point of view, ought to cure it—nevertheless caused an exacerbation, the explanation was to be found in the fact, that to the original disease there was superadded a medicinal disease produced by an overdose. As a remedy for this he propounded the doctrine of the potentialization of remedies—that is, the development of their truly curative effects by so diluting the drugs that all their material properties were done away with, and only the curative essence or spirit remained. And here he introduced his ridiculous theory that effectually to potentialize any given drug, only a certain number of rubbings and shakings for each dilution could be permitted, for fear of developing too powerful an action—as if a curative action could be too powerful. His followers pushed this doctrine of potentialization to such an extreme, that, in their dread of producing a medicinal exacerbation, some of them only employed the 800th or 900th dilution, and found it quite sufficient to let their patients smell the globules impregnated with these solutions once in four weeks or so. Hahnemann, however, speedily found that, however useful in acute diseases, all these specious theories failed him in the treatment of chronic disease, and he then developed quite a new doctrine—his psora theory—in which, ignoring the all-sufficiency of the so-called law, "similia similibus curantur," he propounded the dogma that remedies for chronic disease were not to be selected according to that principle alone, but were to be taken from a class which were supposed capable of producing psora, or itch—scabies—to the presence of which latent in the system all chronic diseases not syphilitic or sycosic in character were supposed to be due. We need scarcely say, that the discovery of the itch insect, and the rapid cure of scabies by poisoning this mite, gave the *coup de grace* to the psora theory; and bit by bit, since Hahnemann's death, the whole fabric which he built up has gradually come tumbling down, till now, as Hempel says, "pure homœopathy," or Hahnemannism, is almost unknown. The dogma, "similia similibus curantur," known and recognised in all ages as possessing a certain limited value, has most signally failed when propounded as an exclusive principle for the selection of remedies, even in the hands of that great "double-head of erudition and philosophy," as Jean Paul Richter termed him, who spent his life in extending, and to the best of his ability perfecting, this theory. Those who still call themselves homœopathists do so for the sake of the notoriety which is attached to the name; but as Hempel shows (and his work is worth reading in this respect), they are gradually casting from them all that distinguishes them from their medical brethren; the psora theory, medicinal aggravation, and infinitesimal doses, have all been thrown overboard, and they are busy working back to the old tracks, guided by Kopp and Rademacher, on the devious and uncertain path of specific medicine. Legitimate medicine, on the other hand, which had long been wandering on treacherous ground, led astray by the erratic glare of erethism and phlogosis, is now beginning to tread firmly on the sure basis provided by physiology and pathology. The "Young Physic" of John Forbes has his locks now streaked with gray, yet, led by the kindly hand of science, he may look back with thankfulness on the dangers he has escaped in the past, and forward with no fears for the future. The destined victory foreseen by Forbes is already won—Hahnemannism is no more.

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[1] Odyss. *ρ.* 217, 218. Opp. ex recensione Samuelis Clarkii curavit, J. A. Erneatus, Glasguæ, 1814. Tom. iv. p. 151.

[2] Lysis. Opp. edit. bipont. Biponti, 1787. Vol. v. p. 233.

[3] Sect. iii. p. 310.

Hempel is a well-known, and in some respects a representative man, and his work is well worth reading as affording an authoritative record of the present standpoint of homœopathy, but as yet there is no common ground on which we can meet. Wide as he has strayed from Hahnemann, his views and opinions are still too devoid of substantial basis to permit of any profitable controversy,—they must either be accepted or rejected. How can we argue with a man who calls himself a homœopathist, and yet treats chlorosis with very material doses indeed (p. 21) both of iron and digitalis—remedies which we all acknowledge to be well calculated to do good in such a disease, but which in no man's hands have ever produced the disease they so readily cure; which are therefore no more homœopathic to chlorosis than colocynth is to constipation. And it would be even still more absurd to attempt to argue with one who supposes himself to be a reasonable being, and who yet believes that he has cured a disease with one globule of the 200th dilution of ignatia, upon which one drop of the strong tincture of the same drug had no effect whatever (p. 92). The 30th dilution is equal to one drop of the mother tincture diluted in an ocean of fourteen septillion cubic miles of fluid; that is, in a quantity sufficient to make one hundred and forty billion spherical oceans extending from limit to limit of Neptune's orbit, or as much as would make many hundred spheres, each with a semi-diameter or radius in length equal to the earth's distance from the nearest fixed star. What the 200th dilution may amount to we know not; our readers who have leisure may calculate it, or those with strong imaginations may endeavour to conceive it, but we feel sure that no sane man could for one instant believe that a globule of the 200th dilution of anything could have any effect whatever—least of all in a case where the administration of one drop of a drug so immeasurably more powerful as the mother tincture was wholly resultless.

Many years ago, a highly respected citizen left the cold shades of Presbyterianism for the more congenial quarters of the Romish Church. His clergyman, who had a great respect for him, asked him what had induced him to take such a step; the reply to this was, "Reading the lives of the saints," and to the rejoinder, "Will you lend me them?" "Certainly." A short time after, the clergyman returned, and opening the portly tome at certain parts descriptive of sundry apparitions of the Virgin Mary, and of certain remarkable miracles, he demanded of his friend, "Do you believe all these statements?" "Most assuredly." "Aweel, aweel, ye may be a Roman if ye like, but my faith is no sufficient." What other reply is possible to Hempel? whose statements and pretended arguments are so baseless and irrational as to repel every inquirer with even the slenderest claims to mental sanity.

As for Dudgeon's pamphlet, the most satisfactory part of it is that in which he says, "We cannot now look for any increase of the numbers of avowed followers of Hahnemann;" and if we believe what he adds, that "the boundary line between homœopathic and orthodox practice is daily becoming less distinct," it is certainly not for the same reasons as he does. Not because legitimate medicine has in any respect adopted the views of Hahnemann, but because the present followers of homœopathy have rejected all Hahnemann's peculiar views, as Hempel testifies. The history of medicine is amply recorded, and it is only ignorance of that history which would prompt the inditing such a pamphlet,—an ignorance which could only be paralleled by the astounding ratiocination of the appendix, in which he argues—to take one item—that because the deaths from apoplexy numbered under homœopathic treatment in Philadelphia only 28, while under what he terms allopathy they were

162, the number of allopathic physicians being four times that of the practitioners of homœopathy, therefore homœopathists are more successful than allopathists in the treatment of that disease in the ratio of 28:40; leaving entirely out of view the very probable contingency, that the homœopaths killed all they got. We all remember the Eastern tale of the spirit-seer seeking a doctor through the streets of Bagdad: Passing the doors of those medical men whose steps were crowded with ghosts, he stepped with alacrity into the consulting-room of him at whose entrance stood one solitary spirit, certain that his faculty had enabled him to select the most successful practitioner in the city. And we also remember the welcome he received as the second patient who, during twenty years, had sought the illustrious medico, as well as the horror with which spirit-seer himself hurriedly retreated.

On Functional Derangements of the Liver. By CHARLES MURCHISON, M.D., LL.D., F.R.S., etc. London: Smith, Elder, and Co.: 1874. Pp. 182.

THE lectures which compose this little book were the Croonian lectures delivered before the President and Fellows of the Royal College of Physicians, London, in March 1874; they were subsequently published in the *Lancet* and *British Medical Journal*, and are now republished with alterations and additions. They are well printed and agreeably composed, but most sketchy in character; and though they afford further proof of what we all already know full well, that Dr Murchison is a well-read man and an agreeable writer, they will not add to his reputation as a scientific physician. We are glad to see that, notwithstanding the adverse report of Dr Bennett and his mercury committee, Dr Murchison still believes that the proofs of the efficacy of mercury in certain derangements of the liver are overwhelming. In this we think most physicians will agree with him. We can recommend the book so far as it goes; but an organ so important as the liver deserves to have its functional derangements more thoroughly treated than they have yet been.

The Pharmacopœial Companion to the "Visiting List:" A Posological Table of all the Medicines of the British Pharmacopœia, arranged according to their Action. By ROBERT T. H. BARTLEY, M.D. London: Baillière, Tindall, and Cox: 1875.

THE title of this "Companion" sufficiently indicates its nature. The medicines of the Pharmacopœia are arranged in 21 groups;

and when a medicine has more actions than one, it is mentioned in different groups. On the whole, the medicines are well arranged, and the doses correctly given; and the "Companion" is admirably adapted for being inserted into the "Visiting List." In some cases, however, the doses are not correctly given; for example, the dose of the perchloride of mercury is put down at gr. $\frac{1}{16}$ to gr. $\frac{1}{4}$, instead of gr. $\frac{1}{16}$ to gr. $\frac{1}{8}$, as given in the British Pharmacopœia; on the other hand, the dose of the subchloride of mercury is given as gr. $\frac{1}{2}$ to gr. i., instead of gr. $\frac{1}{2}$ to gr. 5. Again, we would consider *one drachm* too large a dose of the *nitrate of potash*. With regard to the grouping of the medicines, we find some inaccuracies. The first medicine mentioned under Diaphoretics is carbolic acid, with a dose of gr. i. to gr. 3. Few practitioners would ever think of prescribing this medicine in such doses as a diaphoretic. Under *Purgatives* we find nux vomica and its active principle strychnia. Surely these are not purgative in any true sense of that term. One only other remark is, that it is defective, inasmuch as it omits those important drugs contained in the *Addendum* to the British Pharmacopœia of 1874, one of the most important of which is the hydrate of chloral. We hope to see these defects remedied in the "Companion" for 1876.

A Treatise on Pharmacy. By EDWARD PARRISH, late Professor of the Theory and Practice of Pharmacy in the Philadelphia College of Pharmacy, etc. Fourth Edition. By THOS. G. WIELAND. Philadelphia: Henry C. Lea: 1874. London: Trübner and Co.

WE are glad to find that this great work of Professor Parrish has reached a fourth edition. The author died whilst this edition was being prepared, and, after his death, it was completed by T. G. Wieland of Philadelphia.

The merits of this work are too well known to require commendation from us. This *Treatise* will long remain a standard work on Pharmacy. This edition has been much enlarged, and thoroughly revised by the editor. It is specially meant to illustrate the United States Pharmacopœia, but will also be found to be a most valuable work of reference to all in this country who are interested in pharmacy. The work is worthy alike of its author and of the important subject of which it treats—a subject far too little studied by many members of the medical profession.

Persecution for Opinion in Medicine. London: Henry Turner and Co.: 1874.

THIS is a reprint, with additions, from the *British Journal of Homœopathy* for October 1874. The first part of this pamphlet is occupied with an account of a controversy between the Editor of the *Medical Press and Circular* and Dr Dudgeon, President of the British Homœopathic Congress of 1874, regarding the exclusion of homœopathists from consultation with members of the medical profession. The latter half of the pamphlet is a vindication of homœopathic principles. It is no part of our present intention to discuss at length the theory and practice of homœopathy; we would simply assert that the whole tone of this paper will tend to widen the breach that exists between homœopathy and the medical profession. It is pure presumption to assert that allopathists "add to their materia medica by unscientific empirical trials of new drugs on the sick, or *or by pilfering from the stores of homœopathy.*" The author of this pamphlet assumes, without any proof, that homœopathists *alone* practise on a scientific basis, and are free from empiricism; but we must believe the very opposite until homœopathists shall demonstrate by experiment that medicines diluted to *billionths* are capable of producing any effect on the animal economy, far less those marvellous effects ascribed to them by "extravagant dilutionists." It is all very well to speak of their own pet theories as scientific; but we would remind them that there is such a thing as "science falsely so called."

Dr Pereira's Elements of Materia Medica and Therapeutics. Edited by ROBERT BENTLEY, M.R.C.S., Professor of Botany in King's College, London, etc., and THEOPHILUS REDWOOD, Ph.D., Professor of Chemistry and Pharmacy to the Pharmaceutical Society of Great Britain. With an Appendix, containing the New Medicines included in the additions to the British Pharmacopœia of 1867, published by the Medical Council in 1874, and Commentaries thereon by the Editors. London: Longmans, Green, and Co.: 1874.

THIS work is an abridgment of the great work of Dr Pereira on Materia Medica, than which no book in the English language contains more information concerning the natural history, the chemistry, and the actions and uses of drugs, at the time it was written.

It is now more than a quarter of a century since Dr Pereira died; and although his great work is still full of the most valuable information, it became necessary to adapt it to the British Pharmacopœia; and accordingly we have the volume before us. This work is by no means limited to the officinal preparations of the British Pharmacopœia, but includes most medicines which are held in high esteem by medical men wisely or unwisely. It also contains an "Appendix" in which is given an account of those medicines which are contained in the "Addendum" to the British Pharmacopœia published in 1874.

Professors Bentley and Redwood have performed their task well; and we have in these "Elements" an amount of information on Materia Medica and Therapeutics unsurpassed in any book in our language. The book is well written, and contains many illustrations, which are beautifully executed. It will form an invaluable acquisition to the library of any member of the medical profession. The publishers have done their duty well, except the fact that they have published the work in one volume. As the volume extends considerably over *one thousand* pages, it is a pity that it was not published in *two* volumes. It would have formed two fine volumes, which would be a great advantage over the present *one* volume edition. We hope that a new edition will soon be necessary, and that we shall then have it in *two* volumes.

An Investigation into the Action of Veratrum Viride upon the Circulation. By H. C. Wood, jun., M.D., assisted by Jos. Berens, M.D. Reprinted from the *Philadelphia Medical Times.*

This is a very valuable paper, giving an accurate account of many experiments upon animals with the two alkaloids of veratrum viride. Veratria, the active principle of this plant, is now known to be a complex body, and contains two alkaloids, sabadillia and jervia. It is with these two alkaloids that Dr Wood performed his experiments. He calls the alkaloids veratroidia and viridia, the latter of which he has proved to be identical with the jervia of some authors. Dr Wood's veratroidia must be regarded as the sabadillia of others. With each of these alkaloids Dr Wood investigated the action on the circulation, on the heart and its nerves, and on the vaso-motor system.

Regarding the action of veratroidia, Dr Wood thus concludes:—"The action of this alkaloid upon the circulation is altogether subordinate to its influence on the respiration. In minute doses it stimulates the cardiac inhibitory nerves or nerve-centres, but when given in sufficiently large doses it finally paralyzes the peripheral inhibitory cardiac nerves. Upon the vaso-motor system, veratroidia acts as a depressant, but its influence is feeble, much less intense than its action upon the pneumogastrics. When artificial respiration is maintained it can be given in such doses as to paralyze the vaso-motor centres."

Regarding viridia he thus sums up:—"Viridia, in its action upon the circulation, as compared with its influence upon the respiration, is very much more powerful than veratroidia. The slowing of the pulse and the lowering of the arterial pressure, caused by viridia, are due to a direct action upon the cardiac muscles and upon the vaso-motor centres, upon both of which the alkaloid acts as a powerful depressant; upon the inhibitory and accelerator nerves of the heart, viridia acts not at all, or so slightly that its influence is not perceptible. Veratroidia lowers the pulse rate by its action upon the inhibitory centres; viridia lowers the pulse-rate, as it were, by benumbing the muscle."

We highly commend this elaborate and valuable paper.

Chemistry in its Relations to Therapeutics. By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.S.E., etc. London: Baillière, Tindall, and Cox: 1874.

This is a reprint from the *Medical Press and Circular* of a paper by Dr Griffiths, in which he states that his "object is to direct attention to certain recent chemico-therapeutical researches which are of no little importance as serving in some degree to raise therapeutics from the grade of irrational empiricism to the dignity of an exact science."

There is not much in this paper to accomplish the noble object of the author. Some of the experiments referred to are anything but recent, whilst others in no way contribute to this end. The author has promised on some future occasion to give some researches in which he is at present engaged; and we expect that in them he will help to raise therapeutics to the dignity of an exact science. And we would humbly suggest to Dr Griffiths that the less frequent use of the first personal pronoun would add elegance to his writings. Surely the occurrence of "I" *five times*, in addition to "my own" in the last nine lines of page 5, is too much of a good thing.

Ueber Zufälle nach Operationen an der Harn- und Geschlechtsorganen. Von Dr JOS. ENGLISCH, Dozent für Chirurgie an der Wiener Universität. Separat Abdruck aus der "Wiener Mediz Presse" 1873.

THE author of this pamphlet relates with great minuteness the history of fifteen out of eighty cases, observed by him, of stricture of the urethra, with the view of determining the nature of the febrile symptoms which frequently follow the introduction of the catheter. He comes to the conclusion that, in the great majority of cases, they are not due to the mere dilatation or tearing of the stricture, nor, as maintained by some, to the absorption of the urine in its passage over the torn urethra, but to the excitation of the kidneys, propagated from the stricture along the bladder and ureters to these organs, and resulting in nephritis. He points out that urethral fever has its intensity and fatality determined, not by the condition of the stricture of the urethra, but by the state of the kidneys. When the kidneys are healthy, a catheter may be passed into the bladder through a tight stricture with considerable violence without febrile symptoms necessarily ensuing. If, however, the kidneys be in a diseased condition, the passage of the catheter through a comparatively slight stricture may give rise to a violent febrile attack which may end fatally. It follows from these observations, that when the kidneys, as may be ascertained by an examination of the urine, are in an irritable or diseased condition, it is the duty of the surgeon to abstain from catheterization, unless obliged to relieve urgent symptoms of retention. We commend this publication to the favourable notice of surgeons as dealing with a subject to which perhaps less attention has been paid than its importance deserves.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING IV.

Wednesday, 3d Feb.—Dr D. RUTHERFORD HALDANE, *President, in the Chair.*

The following Patients were shown :—

I. *Dr P. H. Watson* showed first A CASE in which, in the month of August 1874, he had excised the entire carpus and base of the metacarpus by his own method, which he had formerly, upon more than one occasion, described to the Society. His reason for showing this result was that Dr Gillespie, in a note to a paper presented to the Society, seemed to hint that although Dr Watson had shown several specimens of this mode of operation, as he had not showed any case illustrative of a successful result, his claim to originality might be regarded as doubtful. He presented this case only because he had chanced to see her to-day at hospital, and had thus an opportunity of arranging that she should come here to-night.

II. *Dr Finlay* showed—(1.) A BOY, who, for sixteen months, had suffered from suppurating inguinal glands. The cause was ultimately discovered in an ingrowing toe-nail, after the removal of which the patient did well. (2.) A YOUNG WOMAN with a forked uvula.

Exhibition of Pathological Specimens :—

III. *Dr P. H. Watson* showed—(1.) A MODIFICATION OF THE URETHROTOME which he had exhibited on a recent occasion to the Society. The movement by which protrusion of the blade was effected consists of a screw acting upon a conducting rod, on the same principle as that by which the male blade moves within the female blade of the lithotrite. This modification renders the withdrawal of the blade as uniform in its movement as its protrusion. The instrument is a Syme stricture staff, from which a slender tenotomy knife is made to project at an angle to the shaft behind the shoulder of the instrument. The object of the shoulder is to arrest the passage of the instrument at the anterior aspect of the constriction, and thus to secure that the cutting part of the instrument corresponds accurately to the site of the stricture. The stricture is divided from behind forwards as the instrument is withdrawn.

(2.) PROLIFERATING CYST OF THE MAMMA, removed, along with the mammary gland, from a young lady. The case was one in which, when seen two years ago, the disease was cystic. Dr Watson had then recommended the removal of the breast. Elsewhere the case had been treated by seton, with the result of producing this endogenous growth, about the size of an orange, from the interior of the cyst. The growth had a soft, elastic consistence, a pale fawn colour, and a leaf-like arrangement of its component parts. When the endo-cystic growth was hardened by chromic acid, and examined after tinting with carmine, it was found to present all the characters of adenoma.

(3.) A SARCOMA removed from the scalp, with a multitude of small ovoid and granular miliary masses, due to the inclusion of the sebaceous elements of the skin within the sarcomatous growth.

(4.) The CARPUS AND METACARPUS from a man in whom last year Dr Watson had removed the trapezium and trapezoid bones. Recently the inflammatory affection extended to the remaining osseous parts and to the carpal joints. The bones now shown were removed through a single linear incision.

(5.) The parts illustrative of INTRACAPSULAR FRACTURE OF THE NECK OF THE THIGH-BONE from a female, æt. 65. Death occurred somewhat unexpectedly from pulmonary embolism. The ligamentum reflexum was completely torn through upon its anterior aspect, but was complete upon the posterior aspect of the neck of the bone. There was considerable thickening of the capsule of the joint upon the posterior aspect of the joint.

IV. *Dr Littlejohn* showed—(1.) A specimen of RUPTURE OF THE HEART. A man fell accidentally from the roof of one of the tallest houses in the old town of Edinburgh (eight storeys), and was killed on the spot. The only external mark of injury was a laceration of the scalp in the centre of the forehead. The cranium was intact, and the brain, with the exception of slight sub-arachnoid effusion of blood, presented a normal appearance. The right cavity of the chest contained several pints of fluid blood, which had escaped from several apertures in the right auricle. There was universal adhesion of the pericardium from old-standing inflammation. The heart and aorta were of normal size, and the cardiac substance was microscopically healthy. The liver was slightly ruptured on its under surface. The cavity of the pelvis contained about a pint of fluid blood, which had apparently come from a wound of the iliac vein, the result of a detached portion of the side of the sacrum being driven against it. There were no other injuries.

(2.) The WINDPIPE AND PORTIONS OF THE SKIN OF THE NECK from a case of suicide by hanging. The dark parchment-like appearance of the furrow, caused by the pressure of the ligature, was well seen, although the ligature employed was a soft cotton handkerchief. The skin over the thyroid cartilage showed a marked cicatrix, the remains of a suicidal wound inflicted seven years previously. Internally the reparation of the wound was most complete, there being scarcely any vestige of a cicatrix.

V. *Mr Joseph Bell* showed—(1.) A small specimen of EPITHELIOMA removed from the lip of an aged female. He remarked that he had now operated seven times for epithelioma of lip in women, and that in every case the patient had been a smoker. He could only account for this greater proportion, as regards females compared with males, on the supposition that the dirt, shortness, and therefore heat of the pipe used, acted as an irritant, as women, trying to conceal a habit of which they are often ashamed, use a short pipe.

(2.) Also TWO TEETH, with plate attached, which had been swallowed by their wearer, a young female. He had been suddenly called out to attend her, as she was supposed to be dying from suffocation. On reaching the house, however, he found that the spasm had passed off. On making inquiry, he found out that her false teeth had become loose, but yet she had foolishly neglected to take them out on going to bed. By passing a probang into the stomach he hit upon a hard body, which he had no doubt was the teeth. Notwithstanding Mr Little's success in directly extracting them from the stomach, he did not try to do so—more especially as the gold plate was sharp-edged. He therefore ordered the patient to keep her bed, and take as indigestible a diet as possible, consisting of cheese, porridge, suet dumplings, stewed apples, etc., along with opium occasionally. By this means large solid stools were produced, in one of which the teeth had been passed by the patient, notwithstanding the size and jagged outline of plate, without any pain or damage to the alimentary tract.

VI. *Dr Haldane* showed a GALL-BLADDER which had undergone calcification. The subject from whom the specimen had been obtained, a woman aged 53, was admitted into the Royal Infirmary suffering from ascites, pain in the abdomen, and vomiting. Her statement was, that she had been in good health until about a year before, when she began to suffer from pain in the stomach after taking food, and vomiting. When admitted, the circumference of the abdomen was 39 inches; there was well-marked fluctuation, with distension the superficial veins and lymphatics. At this time palpation did not reveal the existence of any tumour. As the accumulation of fluid caused much distress, she was tapped shortly after admission, and 220 ounces of a clear yellowish fluid, having a specific gravity of 1020, were discharged. A distinct hard ridge, between two and three inches long, could now be felt in the epigastrium, running in a transverse direction. Two or three hard nodules could also be felt at other points below the abdominal parietes. The tapping gave much relief, but as the fluid rapidly re-accumulated, it required to be repeated in about a month. From this operation she did not rally, but gradually sank, and died a week afterwards. On post-mortem examination, several cancerous nodules were found in the peritoneum, parietal and visceral. The great omentum was shrivelled up, and converted into a cancerous mass. The stomach was in an advanced state of cancerous degeneration. When the distended intestines were pushed aside, an enlarged and perfectly hard gall-bladder was seen to project about three inches beyond the lower border of the liver. This gall-bladder was as large as a moderately-sized French pear; its coats were almost entirely converted into calcareous matter. The commencement of the cystic duct was completely obstructed by a gall-stone of the size of a small filbert, and the duct beyond was converted into a fibrous cord. The gall-bladder was filled with a greenish flakey substance of the consistence of Devonshire cream, which, on microscopic examination, contained an immense number of granules, and crystals of cholesterine. Besides this matter, there was in the interior of the gall-bladder a single gall-stone of the size of a small chesnut. The peritoneal coat of the liver in the portal fissure was converted into cancerous matter, and must have exerted pressure upon the portal vein. After the post-mortem examination, the patient's daughter was applied to, and stated that, five years ago, her mother had suffered most severely from pain in the belly and vomiting, and had been laid up at that time for six weeks. There had been no jaundice then or subsequently. Dr Haldane observed that the sequence of events appeared to have been this: calculi had formed in the gall-bladder; one of these had engaged itself in the commencement of the cystic duct, but, having been unable to pass, had remained impacted there. Inflammation of the mucous membrane of the gall-bladder had been set up, leading to suppuration, and thickening of the coats of the bladder. The thickened coats had gradually undergone calcareous degeneration, and it was very possible that this had prevented the abscess from bursting. The ascites had evidently been due to pressure upon the portal vein by the cancerous peritoneum. The hepatic ducts had however escaped, as jaundice had never been present.

VII. *Mr Annandale* showed—(1.) An OS CALCIS very much comminuted, which he had excised in the case of a man who had fallen from a height of 40 feet. The interest of the case was, that the patient had sustained no other injury, the whole force of the fall having been expended on the heel. The patient was making good progress, and Mr Annandale expected that he would recover with a useful foot.

(2.) An example of a RUPTURE OF THE LIGAMENTUM PATELLÆ, obtained from a patient who had received a violent twist of the limb. The ligament was completely torn away from the patella, and a portion of the periosteum of this bone had also been torn away.

(3.) The parts removed in EXCISION OF THE ELBOW-JOINT from a case of unreduced dislocation of both bones of the forearm backwards. Reduction was found to be impossible, owing to a fracture and adhesion of the coronoid process of the ulna, and therefore excision was performed.

(4.) A specimen of HÆMATOCELE, in which the tunica vaginalis was completely filled with solid clot. The testicle was compressed, and lay at the bottom of the tumour.

(5.) A CALCULUS removed by forceps from the urethra of a man. The calculus was freely movable in the urethra, and had been lodged there for nearly two years. It only occasionally interfered with the flow of the urine.

VIII. *Dr Foulis* read a paper on CANCER OF THE OVARY. He thought it necessary to say something first on the anatomy of the organ, particularly in reference to the development of the Graafian follicles.

Waldeyer, whose beautiful work on the Development of the Ova and Ovary was published at Leipzig in 1872, was the first to describe the germ epithelium and show its relation to the stroma of the ovary. He showed that by a peculiar growth of the vascular stroma on the one hand, and of the germ epithelium on the other, large and small rounded masses of the latter became embedded in the stroma of the ovary, and that several, sometimes many, of the cells in each embedded mass became conspicuous by their size and the size of their nuclei, while the other cells keep to their original size and surround the larger ones as a kind of epithelial investment. The large cells Waldeyer calls primordial ova. Processes of vascular stroma then grow into each embedded mass of epithelial cells between the primordial ova, enclosing them, along with their epithelial investment, in separate meshes of the stroma, and in this way Graafian follicles are formed. The small cells surrounding the primordial ova form the follicular epithelium cells. According to Waldeyer, a genetical distinction between primordial ova and follicular epithelium cells has consequently no existence; the germ epithelium is the common source of both.

Dr Foulis then stated shortly the result of his own investigations on the development of the ovary and Graafian follicles.

In the human fœtal ovary he had been able to trace that, by the growth of processes of stroma up between the germ epithelial corpuscles, large and small masses of the latter become embedded in the stroma of the ovary in the manner described by Waldeyer. All the embedded corpuscles are potentially ova, though many of them never reach that stage of development. The stroma of a seven months' human fœtal ovary consists almost entirely of connective tissue corpuscles. Little processes of the stroma made up of such corpuscles grow in between the embedded ovigerms, and as the latter become conspicuous by their size, the processes thicken between them, and at last each young ovum is gradually shut up in a separate mesh or Graafian follicle. The epithelium of the follicles or tunica granulosa is produced in the following way. As each young ovum becomes enclosed in a mesh of the stroma in the way just described, connective tissue corpuscles in the walls of these primordial follicles lie in close contact with the protoplasm or yelk substance which is produced round the germinal vesicle of each ovum during its development. The nuclei of these connective tissue corpuscles swell up, and by constant division at last produce a perfect capsule round the ovum, consisting of small cells. In all parts of the ovary, wherever germ epithelial cells are developing into ova, connective tissue corpuscles are found lying in close contact with their protoplasm or yelk substance, and the nuclei of these corpuscles swell up, and by constantly dividing, produce the small cells which constitute the follicular epithelium or tunica granulosa of the Graafian follicles. Dr Foulis thus shows that, while all the ova are derived from the germ epithelium corpuscles, the follicular epithelial cells are part of the stroma, and are derived from connective tissue corpucles in the stroma of the ovary. At birth the human ovary contains not less than 30,000 Graafian follicles, and the epithelium cells of these are produced by the proliferation of connective tissue corpuscles in the stroma of the organ.

As the result of many investigations which he has made on the development of cystic tumours of the ovary, Dr Foulis is of the opinion that although, in most cases, over-distension of Graafian follicles is the starting point of cystic tumours of the ovary, when the disease has once started, there is a formation of new cysts in the diseased organ which may go on to an unlimited extent. He has carefully traced the development of small cysts, often found in great numbers in the walls of parent cysts, and he believes that the epithelium of these little cysts is produced by the proliferation of connective tissue corpuscles in a manner very similar to that which takes place in the formation of the epithelium of healthy Graafian follicles. After stating that every cyst, however small, in the diseased ovary had a distinct epithelial lining, he read the following short notes of two cases of cancer of the ovary to show how important a part is played by the epithelium of cysts in the production of certain forms of malignant disease of the ovary. The first case was that of an Irish lady who consulted Dr Thomas Keith early in

October about a tumour of the abdomen. Dr Keith, on examination, discovered a semi-solid tumour, larger than an adult head, surrounded by much ascitic fluid. The belly was tapped, and about two gallons of fluid tinged red were drawn off. The sediment from this was carefully examined under the microscope. On placing a drop of it on a glass slide, numerous small white bodies, varying in size from a pin's head to minute specks, were seen with the naked eye; these under the microscope were found to be little masses of epithelial cells and nuclei, proliferating in an extraordinary manner; epithelial cells in all stages of development were projecting from the little masses. From the close resemblance of these cells to the cells of epithelium lining young cysts of an ovarian tumour, Dr Foulis thought the little masses were fragments of epithelium which had escaped from small burst cysts, and, floating free in the ascitic fluid, were proliferating. The patient improved much after the tapping, and ovariotomy was performed. A fungous mass the size of an egg was found growing from the wall of the pelvis on the right side, and the tumour itself was one large malignant mass. Some small burst cysts were found on the surface of the tumour, and many small cysts unbroken, and filled with large epithelial cells, were found in different parts of the tumour. In some of the cysts the epithelium was much thickened and projected in papilliform growths.

The second case was that of a woman about thirty years of age, who in September consulted Dr Thomas Keith about a tumour in the abdomen. Dr Keith discovered a large cystic tumour of the ovary, surrounded by a considerable quantity of ascitic fluid. The patient's right pleural cavity was filled with fluid. Dr Keith first tapped the abdomen, and three days after this drew off from the right chest 127 oz. of serum. A microscopic examination of the ascitic fluid showed a large quantity of cells and free nuclei from cysts of the ovarian tumour. On the 3d October, Dr Keith again drew off from the abdomen 32 lbs. of fluid. The deposit from this was carefully examined. With the naked eye many small white bodies were seen, and these under the microscope were found to be little masses of proliferating epithelium, similar to those found in the Irish lady's case. On the 27th November the abdomen was again tapped, and the fluid emptied into a large foot-pan. In the course of a couple of hours the deposit was an inch thick, and, without exaggeration, half of the deposit consisted of these little masses of sprouting epithelium, they had increased so enormously in quantity! At this time, patient was remarkably well in health, and on being asked as to the state of her health, always replied cheerfully, "Oh, I feel quite well!" She showed no signs externally of internal malignant disease. From the experience we had gained by the Irish lady's case, Dr Keith made up his mind that this was not a case for operation. The patient soon went home to Perth, and on 28th December died suddenly. Through the great kindness of Dr Absolom, of Perth, who performed a most careful examination of the body, I received an account of the condition of the abdomen, and also the whole tumour. The peritoneal surface everywhere was studded with small white nodules of a malignant nature. The tumour weighed about 12 lbs., and was of the right ovary. The Fallopian tube was drawn out to the extent of 14 inches along one side of it. The mass was made up of a number of cystic growths, varying in size from a marble to a fœtal head, all fused together and presenting various degrees of solidity. In all the cysts the epithelial lining was enormously thickened, in some cases to the extent of an inch and a half. The internal surface of each cyst was rough like a cauliflower growth. In the centre of the tumour was a semi-solid malignant mass the size of a child's head. In every part the tumour was found to be of a malignant nature. In bringing these two cases before the Society, the object Dr Foulis had was to direct attention to the little masses of sprouting epithelium found in ascitic fluid surrounding a tumour in the abdomen, as a means of diagnosing malignant ovarian tumour and malignant peritonitis. Dr Foulis has no doubt that the malignant disease in both these cases originated in the ovary, and that the malignant condition of other parts in the abdominal cavity was caused by the dissemination of elements from the primary seat of the disease upon neighbouring parts. In the second case, it was not possible to say from the external signs that the patient was suffering from internal disease of so grave a character; and yet after death at least 10 lbs. of cancerous material was found in her abdomen. No doubt, a malignant ovarian tumour may be present in the abdomen without the presence of these little masses of sprouting epithelium in ascitic fluid, but where ascitic fluid is present, and these little bodies are found in it floating free and growing at a rapid rate, quite independently of surrounding structures, Dr Foulis considers them to be a certain sign of malignant ovarian tumour and peritonitis.

Dr Haldane remarked that the Society was greatly indebted to Dr Foulis for his able paper. In addition to its being an important contribution to the diagnosis of malignant growths, it also showed how valuable were the applications of physiology to pathology, and corroborated the fact, and was an admirable illustration of the doctrine of inoculation of tubercle and cancer, as shown in experiments with guinea-pigs.

Dr Matthews Duncan congratulated Dr Foulis on the ability displayed in his paper, and also congratulated the Society on the acquisition they had made in electing him a member. Although one of their youngest members, he had already established a reputation for himself by his physiological researches. He himself had now been connected with the Society for about thirty years, and he distinctly remembered that, at the beginning of this connexion, there was a discussion in the Society on a similar subject. The question, however, was not the diagnosis of ovarian cancer, but of ovarian dropsy. In this discussion, Dr Handyside, the late Professor Simpson, Professor Spence, and Professor Bennett took part. The last-mentioned gentleman proposed to settle it by examination of the fluid. This proposal, however, did not gain much confidence, and we are still without means of diagnosing ovarian dropsy by examination of the fluid. Spiegelberg had recently attempted with some success to show that there were differences between ovarian and chronic peritoneal fluid, but still the distinctions did not always hold good. Dr Foulis's observations, however, were altogether on a different footing from those of most of his predecessors. He had begun the inquiry in the right and only perfect way—viz., by first studying the development of the ovary and ovum. There was certainly nothing more remarkable than that in the fœtus, so young as to be only recognisable as human, there should be certain cells on the Wolffian bodies already differentiated for the production of its future progeny. Dr Foulis's researches on the development of the ova and Graafian follicles had already been laid before the Royal Society of Edinburgh; and in them he had indicated views different in many important respects from those of Valentin, Pflüger, and Waldeyer. Of these, the most important as bearing on the subject of his present paper was the origin of the tunica granulosa. An inspection of his preparations, of which an admirable specimen was on the table under the microscope, would, Dr D. thought, convince any one, if not of the certainty of his views, at least of their being more in accordance with actual observation than the view of Waldeyer. In the preparation specially referred to, the connective tissue corpuscles are seen in contact with the protoplasm or yelk surrounding the germinal vesicle. The corpuscles could not be regarded as germ epithelium, or as anything else than the connective tissue corpuscles, and they could be seen to deform the yelk by indenting its surface, there being no germ epithelium between them and the soft yelk. In the pathological aspect of the question, a most important point in the paper was the morbid condition of the connective tissue corpuscles, in relation to the peculiar epithelial masses found in the ascitic fluid, which were evidently the product of ruptured cysts. The diagnosis of malignant ovarian cystoma, by finding in the fluid not only of the cysts, but in the ascitic fluid, those beautiful proliferating masses, had been well shown. These masses were new to Dr D., and they were not easily confused with any other structures, but yet it would be necessary to wait for further verification by observations. It had been asserted by some observers, that the finding of enormous epithelial cells in the fluid from the cysts was diagnostic of malignancy. This was incorrect, as in large healthy ovarian cysts the sediment of the fluids differed, and in those undoubtedly healthy, these enormous epithelial cells could sometimes be detected. In conclusion, Dr Duncan remarked that the paper was one of the highest value, and he hoped Dr Foulis would be able to give them on a future occasion further cases.

Dr Grainger Stewart said that he had been much interested in some of the pathological points brought forward in Dr Foulis's valuable paper, particularly in the statement as to the mode of development of the cysts and their epithelium. Upwards of ten years ago he had satisfied himself of, and demonstrated to some of his colleagues in the Infirmary, the fact that in ovarian tumours the cysts may originate from connective tissue corpuscles. He had succeeded in tracing every stage of their formation, from the mere appearance of the spherule within the corpuscles to the fully developed cyst. He thus could confirm Dr Foulis's statement. But in one respect his results differed from those of Dr Foulis, viz., that he considered the epithelium to be developed from the lining membrane of the spherule as it was being transformed into a cyst, not by a peculiar transformation of neighbouring connective tissue corpuscles as Dr Foulis believed. Should the view advanced by Dr Foulis turn out to be correct, it would be a singularly interesting contribution to the history of connective tissue corpuscles, epithelium, and cysts.

Mr Chiene was also interested in the paper, which raised the great question of the development of epithelium from connective tissue. Carcinoma is now generally regarded as a tumour developed from an epithelial type of tissue; the chief objection to this view is its presence in tissues where no epithelium is normally met with, as in bone and muscle. According to Billroth, in these situations no true carcinoma can exist, because he believes that epithelium cannot be developed from connective tissue. Virchow, on the other hand, believes the contrary. He himself was inclined to agree with Billroth; but if Dr Foulis's views proved correct, this would, in the ovary at any rate, be shown to be wrong, and would go a great way to the settlement of one of the most debated questions in surgical pathology.

Dr Foulis replied. In the development of the epithelium of the Graafian follicles there were first connective tissue corpuscles, and then epithelial cells derived from these. In addition, recent views as to membranes with epithelium were, that a superficial layer of connective tissue corpuscles on the membrane develops into epithelium. In regard to columnar epithelium cells found in ovarian fluids, on the presence of which Spiegelberg lays stress as a means of diagnosing such fluids, he himself had, in examination of about eighty different ovarian fluids, not detected them. In this he had been corroborated by Mr Spencer Wells's former and present assistants, Drs Thomson and Thornton, and also by Dr Drysdale, Professor Atlee's assistant. By means of the diagnostic pointed out in his paper he had been able to predict malignancy of the ovarian tumour in a patient now in Dr Matthews Duncan's ward, otherwise perfectly healthy in appearance. In regard to Dr Grainger Stewart's remarks, he pointed out that there was a proliferation, as well as a swelling out of the connective tissue corpuscles seen in his preparations.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIV.—MEETING III.

Wednesday, 13th January.—Dr MATTHEWS DUNCAN, *President, in the Chair.*

I. An ANENCEPHALOUS FŒTUS was exhibited by *Dr Cairns*, who furnished the following particulars:—The mother is 31 years of age, and has had previously five children, all healthy. Twice the placentæ were adherent, otherwise her labours were natural. During this pregnancy, she complained of being ill, and was unable to go about as she had done on all the previous occasions. The labour lasted four hours. Arms presented. A large quantity of liquor amnii, and a placenta adherent to the fundus. The fœtus presented the appearance of having been dead about a fortnight; the mother also complained of feeling sick and ill from about that period to the time of labour.

II. *Dr James Young* mentioned the following interesting case illustrative of OVULATION taking place WITHOUT MENSTRUATION. Mrs W. was married on the 13th of June 1867, at the age of 25 years. She menstruated on the 13th of July, and her first child was born 4th April 1868. The patient states that she nursed the child thirteen months, and then menstruated six times—till November 1869. The second child was born on 15th of August 1870, nursed five months, when the baby died. From November 1869 till the present time, January 1875, the patient has never menstruated, and during that period a third child was born 31st October 1871, and a fourth child was born on 12th September 1873, and now a fifth pregnancy is going on, each child having been nursed twelve months.

III. ADJOURNED DISCUSSION ON DR CONNEL'S PAPER ON A CASE OF POST-PARTUM HÆMORRHAGE.

Dr Pattison mentioned a case in which life had been saved by compression of the aorta in a bad case of post-partum hæmorrhage, and another in which a grandmother, a mother, and a daughter had each died of hæmorrhage after giving birth to her first child.

Dr Coghill reminded the Society that in hot climates, post-partum hæmorrhage is very common. When he first went to China, he was told that most of the fatal cases of midwifery were due to this cause. He accordingly took precautions to prevent its occurrence in every case of labour which he attended. His method was, (1) to empty the uterus as slowly as possible; (2) to follow up the contracting uterus with the hand, and to have the hand of an attendant always on it, until after the expulsion of the placenta; (3) to put a small padded saucer over the uterus, and a firm binder above it. In a large practice there, he had never lost a case.

Dr Craig said that though the arguments of speakers had mostly been unfavourable to the perchloride, it had undoubtedly stopped the hæmorrhage in Dr Connel's case, while the fatal result could not be attributed to its use. He thought it should be used in suitable cases, and he would himself in a similar case do as Dr Connel had done.

Dr Carmichael thought that perchloride of iron had been raised to far too high a position. The cases of post-partum hæmorrhage could be divided into two classes:—(1) Those immediately controllable; (2) those which are uncontrollable for several hours. He doubted if the perchloride need ever be used if the ordinary measures were thoroughly carried out. Many old and experienced practitioners, among whom was the late Dr Beattie of Dublin, had never seen a death in their own practice from post-partum hæmorrhage. Besides, the perchloride must be injurious to the placental site, and the mucous membrane of the uterus. He had found great irritation produced by it on the nasal mucous membrane when used for epistaxis—probably the same thing happens in the uterus. He concluded that, as a *dernier ressort*, it might be justifiable to use it, but it had too often been employed unnecessarily. He had found irritation of the nipple a valuable aid in stopping hæmorrhage after labour.

Dr Young said that, since the last meeting, he had had two severe cases of post-partum hæmorrhage. He had seen many such cases, but never lost one. In forty years' practice his father had only lost one patient. He trusted to steady, firm pressure, maintained by the left hand over the fundus of the uterus, and administration of ergot and stimulants when necessary. In a case of severe hæmorrhage yesterday, he stated that, while the patient was under chloroform, he removed several clots from the uterus, which, with firm pressure, stopped the hæmorrhage. He thought that the risks incurred in the application of the perchloride were too great to justify its use, and he would require a more clear demonstration of its safety before he would admit it into his practice.

Dr Matthews Duncan said that Dr Connel's interesting paper brought before them an example of one of the most awful events in medical practice, and was fitted and intended to elicit the views of the members of the Society as to the best means of averting this dire result, namely, death from post-partum hæmorrhage. It would scarcely be a misrepresentation to say that Dr Connel's intention was much more restricted, and aimed at eliciting opinions regarding the value of perchloride of iron as a hæmostatic in post-partum hæmorrhage—a remedy to which Dr Barnes had succeeded in giving the utmost degree of prominence. It was not a new remedy, but rather an old one, yet it had of recent years attracted as much attention as if it were entirely novel, and of undoubted and inestimable value. The remarks of Drs Macdonald, Bell, Simpson, and Keiller, Dr D. regarded as of great value, and expressing most of what he had himself to say; but he would venture to give, in his own words, a few of his own reflections on this important practical subject.

When injection of perchloride of iron in solution was recently brought prominently before the profession, and on other occasions, it was prematurely described in terms of injudicious laudation—"No woman now should die of post-partum hæmorrhage," it was said. Unfortunately for the boastings, women had died from post-partum hæmorrhage, apparently more than ever, and even when the vaunted remedies of perchloride of iron and transfusion—both declared to be inestimable and sovereign—had both been employed with skill. The journals had recently had more of these cases than he ever recollected having previously seen. This boasting was for him a bad omen. The best were always modest. He made no doubt that in this instance the boasting was from good motives; and a little boasting or confident speaking in private might be permitted, as tending to secure the confidence of patients, but it was altogether out of place in medical societies and medical journals. Medical history was full of the lamentable wrecks of boasted remedies, and even of such as had had solemn approval, like the *semper, ubique, et ab omnibus* of theologians; and medicine was now, he hoped, too far advanced for any new remedy to secure general confidence, without being able to show satisfactory credentials better than the support of great names and clever hypotheses.

Every one who had seen much of post-partum hæmorrhage, the not very rare cases of extreme and deathlike prostration, with early and complete recovery, and that under all kinds of treatment, would, on reflection, easily understand what a slow and difficult matter it must be to establish the utility of a new remedy. The difficulty and slowness were greatly increased by the rarity of the cases of the worst form—that is, of really imminent danger of death. The recent copious shower of cases in the journals requiring perchloride of iron injection, and when life was supposed to be saved by the use of the remedy, was a very unsatisfactory shower. Dr D. could not avoid making, in regard to these cases, the remark, which had been ably made already with a similar application by Dr Keiller, that it was a pity that many of these poor women had not been simply left alone. He had no doubt of the justice of Dr Keiller's remarks on the *nimia diligentia* of accoucheurs. In his own immediate practice, he had never had a death from post-partum hæmorrhage, but had had ample opportunities of confirming the remarks referred to. He had seen in consultation, and been otherwise connected with, many cases of death from post-partum hæmorrhage. Then he might make a curious remark, that his impression was, that within the last ten years he had known nearly as many deaths from the hæmorrhage of fibrous tumours as from post-partum flooding. In some quarters fatal post-partum hæmorrhage seemed to be common, and imminent danger of it still more so. This supposed common-ness, Dr D. believed, was a result of unsoundness of judgment. In the Maternity Hospital here he believed a death from post-partum hæmorrhage had never occurred.

The only remark Dr D. would make, having special reference to Dr Connel's case, was one to which he attached much importance. It was that, in his (Dr D.'s) opinion, it was not an uncomplicated or frank case of death from post-partum hæmorrhage. With a view to the statement of his opinion, he would name two important complications: namely, first, the tendency to dangerous or fatal swooning or syncope, apart from loss of blood, but in the cases under discussion evoked by parturition, and even a very slight loss, such as under ordinary circumstances would attract no special attention; and, second, previous weakening by disease, loss of blood, or the sickness of pregnancy, predisposing to fatal syncope from slight losses of blood. It was only an opinion, yet one founded on considerable experience, that a majority of fatal cases of post-partum hæmorrhage belonged to one or other of these two categories, and were not simple or frank cases of post-partum hæmorrhage, where the loss caused death of women previously healthy and strong, by its great amount.

He, Dr D., had a vivid recollection of two cases of the former complication. They occurred, as it happened, in healthy young women of splendid physique, and with abundant health, but with a tendency to alarming faintings. In one of them it occurred twice, in her two first deliveries, both under the influence of chloroform. The indications of impending death were so marked in these deliveries, and in the other case, that, with his sanction, the nurse went to call the husband and warn him of the almost certain direful event. In the first case referred to, there were several subsequent successful deliveries without chloroform, one of them with ether as an anæsthetic. The loss in these three deliveries was inconsiderable in measured amount, and was soon completely stopped. Recovery took place under the influence of brandy, administered in moderate doses.

When Dr Connel was reading his paper, Dr D. was struck by the account of the suffering of the patient for many weeks before her confinement, from sickness and vomiting—a condition which must have greatly weakened her, and was described as doing so. The case was an example of the second complication, to which Dr D. had alluded, and it recalled to him several cases which he had seen or been consulted in. The first case of sudden death from post-partum hæmorrhage which he had witnessed, was of this description. An elderly woman, the mother of a large family, suffered intensely from morning sickness before her confinement. The delivery was easy and natural. There was little, very little, loss of blood, but it produced fainting. When he arrived no hæmorrhage was going on, but a small clot was in the uterus. It was easily squeezed out, and the hæmorrhage was stopped, but the woman died before it was possible to do anything further for her than administer brandy. Another case of this kind occurred in a patient, about whose frequent losses of blood during pregnancy he had been repeatedly consulted by a Morayshire practitioner. He recommended the induction of premature labour, but it was delayed and delayed, and at last labour supervened. The delivery was natural, and all was right till after the woman was bound up, and the practitioner had left the room. The nurse, observing a little discharge of blood, recalled the doctor, whose efforts, however, proved vain. In this case, he had received two telegrams in an inverted order. The first was, "Mrs L. is dead." The second was to the effect,—"Come by first train to Mrs L.!" Another case, in the sister of an esteemed medical brother, he need not mention, for it had a very close resemblance to that last described. It was among this second class of cases that Dr D. would place the case of Dr Connel. No doubt, a great part of the cases of grave post-partum hæmorrhage were uncomplicated, and it was his belief that a very great majority of such cases got well completely, or merely suffered subsequently from more or less permanent anæmia. It was a very common thing to hear accoucheurs talk of cases of tremendous loss with no permanent evil results. It was Dr D.'s experience that in most fatal cases little blood—in measured amount—was lost. He very well remembered a case to which he was called by Dr Craig of Ratho many years ago. About 7 A.M. there had been a considerable loss of blood at the commencement of labour, and before the arrival of Dr Craig, who found the patient anæmic, and feeling faint. Dr D. reached the patient soon after noon. The patient was spontaneously delivered about 5 P.M. From the time of Dr Craig's arrival till delivery, no blood was lost, and the patient had recovered entirely from the feelings and indications of faintness. Danger was anticipated from swooning after confinement. When the baby was born, the mother triumphantly declared herself safe and feeling well. The placenta came away as in an ordinary case, and with a loss trifling in measured amount, certainly not above three ounces. The patient was still well; but soon she swooned and swooned again, and died about an hour after the birth of the child, in spite of our best efforts to maintain life.

Whether the post-partum loss was complicated or not, it was supremely desirable to have means of quickly stopping it, and the question of the value of perchloride of iron, which Dr Connel raised, was one of great interest. Dr D. was sure that all would be delighted if the sanguine anticipations of Dr Barnes were realized, for in the treatment of such cases the arrest of the bleeding was the primary, though not the only, indication. Some practitioners quite forgot this, as Dr D. could testify from experience, having found cases treated by the means of arresting hæmorrhage after it had long quite ceased, and while only other indications remained to be fulfilled.

The flooding whose arrestment we were now considering was not from small vessels or an oozing surface, not from any source of spurious post-partum flooding—a kind which was not very rare—but from large vessels, the open sinuses on the surface to which the placenta had recently been adhering. It was the arrestment by a styptic of bleeding from large vessels that we were asked to admit. The arrestment was generally described as the result of a styptic action of the remedy, clots in the sinuses being produced.

Before considering the arrestment of ordinary post-partum flooding by a styptic such as perchloride of iron, Dr D. would say a few words as to the styptic action of this remedy in ordinary bleedings from small vessels. Such were often grave and dangerous. Among them he might mention bleeding post-partum from the lacerated cervix uteri, from the lacerated perineum, from the gums after tooth-extraction, from leech-bites. In such cases as these, styptics were sometimes efficacious, but every practitioner knew how frequently they were useless. Perchloride of iron had been often used in vain to arrest such comparatively slight bleedings, and it was to be remarked, that success in such cases might far more naturally be expected from its styptic action than in cases of ordinary post-partum flooding.

Dr D. fancied that surgeons would not expect an efficient styptic action from perchloride of iron in bleeding from such an anatomical source as that of post-partum flooding; but truly they had no bleedings closely analogous to deal with. The nearest to it—which Dr D. knew well—was the copious oozing from the stump of a lupus mass excised from the pudenda, or of a malignant tumour removed from the same part, or of a cauliflower excrescence.

Now in many such cases in which he had used perchloride of iron, he had found its styptic virtues to be of no avail. It was only in some that it appeared to be followed by a diminution of hæmorrhage.

In gynæcological practice Dr D. had found this styptic, used locally, to be very valuable in cases of metrorrhagia, especially where there was a uterine fibroid causing it or keeping it up. In some analogous obstetrical cases he had found it of use, as in continued bloody losses after abortion. He remembered well a very successful application of this drug many years ago, when the remedy was little heard of. He was called to Dumfriesshire, by Dr Borthwick, to a young woman who had recently aborted in an early month, and who, after continuously losing blood for many days, was, when he saw her, very exsanguine and in a most alarming and dangerous condition. The solution of perchloride of iron, locally applied, arrested the loss and apparently saved life.

The bearing of these remarks was evident—namely, that he did not, from his knowledge and experience in the use of the perchloride, expect much from its styptic action in post-partum flooding. But our knowledge of the matter was not nearly sufficient for the settlement of it by *a priori* reasoning. Direct experience must be the criterion. Besides, he had said nothing of the possible, nay probable indirect action of the remedy by inducing uterine contraction. Ordinary post-partum flooding was not of a kind on which he would expect any mere styptic to have much effect. Till now styptics had never been relied on, and he, Dr D., judging from all he knew or had read, did not place reliance on them even now.

Uterine contraction, and this alone, was the paramount means of arresting uterine hæmorrhage that we could call into action. To call this into action by physical irritation of the muscular fibres of the uterus by kneading and compression, was the quickest and most efficient remedy. A slower means was a dose of ergot, and this had been said to have its action much accelerated by using ergotin instead of ergot, and injecting it hypodermically. These were, in Dr D.'s opinion, worth all other remedies combined, and all he had read of the perchloride of iron injection did not make him think it so useful as kneading and compression of the uterus, and ergot administered internally. No doubt we were now always hearing and reading of cases where the injection was said certainly to have saved life; but Dr D. did not admit the truth of this. It might be true or it might not. There was no way of proving the positive, for every one of experience in losses of blood, whether post-partum or under other circumstances, knew very well that if the patient only survived the arrest of the hæmorrhage, such patient had a good chance of altogether surviving, however alarming and excessive the anæmia and faintness were. Dr D. would add that, when the bleeding was stopped, such cases seemed to get on best when almost completely let alone, or got a little brandy.

It was as much from reading the cases of so-called success as those of failure, that Dr D. had been unable to arrive at a favourable conclusion as to the utility of the iron injection. It was well known that some eminent men—especially Snow Beck—strongly opposed its use, and cited cases where the remedy had apparently caused great evil. But Dr D. did not think these histories sufficient to deter the opposite party, who believed in the value of the remedy, from still farther experimenting with it, and attempting to realize their sanguine and generous hopes.

For his own part, Dr D. said, both theory and extensive experience led him to rely for hæmostasis in these desperate cases, chiefly, though not exclusively, on uterine contraction. To induce and maintain uterine contraction, he would meantime continue to prefer kneading and compression of the uterus, and ergot of rye or ergotin hypodermically used. Not till after these had failed, and till he felt willing to try a doubtful experiment, would he, and then only reluctantly, resort to iron injection. If he ever did this, he would look for advantage from the injection acting as an excitement of muscular contraction, not as a local styptic.

Dr Keiller remarked, that he would be sorry if anything he said at the last meeting were taken as a censure on Dr Connel; he had meant nothing of the kind. He knew Dr Connel too well to believe that he would be injudicious in his treatment of any case. The Society had good reason to congratulate Dr Connel on his being the first to bring prominently forward a case so specially calculated, not only to provoke the incidental discussion which took place at the last meeting, but to induce the President to favour the Society on the present occasion with the carefully considered communication which he had just read. He (Dr Keiller) quite agreed with Dr Duncan in the main, and especially in regard to the difficulty of believing that cases of post-partum hæmorrhage necessitating any such interference were so common as had been recently reported. He thought that the multiplication of such cases in the journals was likely to do mischief, and the bolstering up of a doubtful remedy did much harm to young and inexperienced practitioners, who were only too liable to call any floodings they met with cases of severe post-partum hæmorrhage. He had seen many cases where the danger was gone, and yet the practitioner was still hard at work, trying to stop a hæmorrhage which had ceased—he had done it himself when young. The gushes and clots which frighten inexperienced practitioners rather prove that the uterus is contracting, which is the true remedy, and therefore the object to be aimed at. He could not recall a fatal case in his own practice. His method of treatment was to seize the uterus from the outside in the one hand, pass the other hand into the vagina, and spread his fingers out round the walls of the uterus and cervix (the hand not being put into the uterus itself), so as to have the uterine walls, and therefore the uterine vessels, completely under the control of judiciously applied compression. This measure he had found a very efficient one. He was of opinion, that many of the reported fatal—or supposed nearly fatal—cases would have recovered, had they been so treated. In abortion cases, and in many cases of cancerous erosion, and other local conditions, giving rise to hæmorrhagic discharges, he had used the perchloride of iron, and had found it to answer admirably, but here the dangers to be apprehended from its use were not likely to occur. He believed in its value as a styptic, but not in post-partum flooding, which could be checked quite well without the injection of perchloride of iron, as recommended and practised by Dr Barnes and others, who deem it necessary in such cases. As an antiseptic and most useful corrugant in uterine complaints, he frequently used it.

Dr Connel in reply said, that he was much gratified with the discussion which had taken place. His paper was merely intended as a text for others to preach on. The objections which had been raised to the use of perchloride were reducible to two classes: (1) that it was useless; (2) that it was worse than useless. These objections seemed to be held by the great majority of the Fellows who had spoken, and the unanimity here against the practice, was as remarkable as the almost unanimous deliverance in favour of it in the London Society's discussion. In view of this diversity, he could scarcely think the matter was yet settled. Since the last meeting, he had tried the following experiment: An artery was allowed to bleed into a small saucer containing a mixture of two parts liq. ferri perchlor., and one of water. This mixture was seen to convert almost instantaneously six or eight times its bulk of blood into a tough, hard clot. Now, he could understand how, with a patient in the horizontal position, such a clot might act as an efficient styptic, especially along with external pressure and support. Again, the clot was antiseptic, for he had kept a portion of it in a water-bath, at a temperature of 103° Fah., in contact with some placental blood (kept from coagulating by means of a little alkali), and there was no decomposition for nearly a fortnight. Another portion of the same clot was put loosely into a tube, and a stream of tepid water allowed to percolate over it. No part of the clot was washed off, but the whole was rounded into a glue-like mass, and gradually dissolved. This seemed rather to negative the probability of embolism or septicæmia. Again, he had seen very strong perchloride, with glycerine, used in a case of abortion, with the effect of strongly corrugating the cervix and vagina, and without the least appearance of inflammation or purulent discharge. Death from flooding would still occur in spite of all means to arrest it; and though he did not return from the discussion with greatly increased confidence, still it was evident he would not be condemned for omitting this treatment in any future case.

IV. *The President* communicated a paper by Dr Sutugin, on the MEANS OF ASCERTAINING THE LENGTH OF GESTATION by measurements of the fœtus and gravid uterus during the second period of pregnancy, which will appear in a future number.

Dr Duncan said the paper was so elaborate a one, that it would hardly admit of immediate discussion; but the Society would, he was sure, agree with him, that it was highly honoured in having had communicated to it a paper on which such a vast amount of patient labour and accurate investigation had been expended. He looked upon the paper as one of the highest value.

Dr Coghill moved a vote of thanks to the author, and the Secretary was ordered to communicate with him.

INSPECTOR ELMS.

MRS INGRAM, for aught we know, a midwife of good character, has had, as many experienced practitioners have had, some cases of puerperal fever in her practice (*Obstetrical Journal*, February), and in some of these cases death has resulted. A mighty coroner, whose conduct seems to us reprehensible, sends Inspector Elms to tell her that, if she attended any other case of confinement for two months, she would be held responsible for the consequences!! We hope she replied to this gentleman, requesting him to inform his master that he should mind his own business, as she intended to mind hers. We are not well informed as to English law, but the conduct of this coroner is not consistent with any law we ever heard of in England or Scotland, and it is quite intolerable. Inspector Elms, or the police constable of the beat, may ring loudly any of our door-bells and give us a like message, and with as good reason as Elms addressed Mrs Ingram. If he comes to ours, his master's message shall have a welcome reception, and his master's impertinence a due rebuke, in a more dignified and becoming manner than is implied in a police constable's message.

Down comes the policeman to Sir William Jenner, to tell him to see no more cases of croup for two months, at his peril, as the coroner hears he has been attending some fatal cases; to Sir William Gull, to give up practice for two months, as he hears he has been attending some fatal cases of scarlatina; to Dr Farre, to give up practice, because he has been in repeated consultation in a fatal puerperal case; to Sir J. Paget, to give up operating, as he is in attendance on a case of erysipelas; and so on.

Surely it is no palliation of this conduct to say that Inspector Elms's visit was to a midwife, not a regular practitioner. For us, her inferiority should only ensure the greater consideration. Midwives are a well-known and most useful set of women. There are careless ones, and so are there among the regulars; and at present we are at no loss for an example, for the *Obstetrical Journal* recalls to our minds an awful case, which comes on soon, and which is additionally remarkable from the circumstance that an eminent London obstetrician has at least appeared to defend a man-midwife who clandestinely cuts off a few feet of a living woman's intestines. Such practice, and such an able defender, do not suggest horror of a good honest midwife.

We have read with great pain, on account of its tone, the remarks of the leading weekly journal on the case of Mrs Dymond. The conduct of this journal in attacking Mrs Dymond without due care, and in a malicious style, reminds us of a well-known policeman, who, boldly swaggering with uplifted baton into a street riot, collars a small boy and walks him off to the station. All around Mrs Dymond are regular practitioners doing what Mrs Dymond has done. There are mild words for them, but Mrs Dymond is treated in the severest way. Many of the most eminent practitioners, both dead and living, have done and are doing what Mrs Dymond and Mrs Ingram did, so far as we can judge from the published statements.

We hope our words will reach them when we say that, till otherwise informed, we shall hold them to be good respectable midwives, who have been very unfortunate, and we shall not doubt that their hearts have been torn by agonies of responsibility and sympathy with their patients. Notwithstanding all that Mrs Dymond has had said against her, she may be a deal better and more rational than her critical journalist, whose writings on this subject are as little in accordance with good sense as with good manners.

Should this weekly journal see fit to lay before its many readers sufficient particulars to justify its attack on poor Mrs Dymond—and that there may be such particulars we admit—then we shall say that it should have done this earlier, and that it should have pounced on some man, perhaps an eminent man, not on a poor midwife. There are plenty of men in the midwife's position.

Part Fourth.

PERISCOPE.

M. **Malassez** has given the results of enumeration by his method of the white corpuscles of the blood and their proportion to the red corpuscles in various surgical cases and in women after delivery. We extract the following cases from those published.

A man sustained a compound fracture of the leg and at the same time lost a considerable quantity of blood. The leg was amputated. During the first day he had rigors; abscess formed in the stump; it was opened; the patient improved, and recovered.

The number of white corpuscles during these different periods was as follows:—

	Per millim. cube.	Proportion to red corpuscles.
Before the amputation,	3,500	$\frac{1}{563}$
The 4th day, rigors, abscess,	14,000	$\frac{1}{160}$
The 11th day, improvement,	11,200	$\frac{1}{250}$
The 17th day, doing well,	6,300	$\frac{1}{365}$
The 26th day,	4,200	$\frac{1}{863}$

It is thus seen that the amputation increased the number of the white corpuscles of the blood; an increase that disappeared by degrees in proportion as the cure advanced.

Parturition.—A healthy young woman, aged 22, after a favourable period of pregnancy, was confined without accident. Delivery was followed by several successive insignificant hæmorrhages. They were easily arrested, and the young woman recovered rapidly. She did not suckle.

The number of white blood corpuscles was:—

	P. millim. cube.	Proportion to red corpuscles.
Before parturition,	7,800	$\frac{1}{433}$
12 hours afterwards,	18,900	$\frac{1}{185}$
The 3d day,	13,300	$\frac{1}{231}$
The 10th day,	10,000	$\frac{1}{301}$
The 5th week (after her return to the country),	8,400	$\frac{1}{518}$

Thus, parturition was followed by a rapid and considerable increase of the white blood corpuscles; and this increase gradually disappeared during the following days.

In a woman who died of purulent infection after parturition, the evening before her death the white corpuscles were found:

Per millimeter cube, 62,000: Proportion to red corpuscles, $\frac{1}{54}$.

It is thus seen that after parturition as after amputation, 1st, the number of the white corpuscles of the blood increases rapidly; 2d, if there is no accident, the number of white corpuscles diminishes gradually till it reaches the normal figure; 3d, whilst, if an abscess forms or if there is purulent infection, the number of white corpuscles remains high and can even reach considerable proportions.

Abscess.—Perinephritic abscess. Evacuation by capillary tube. Cure. The number of white corpuscles was—

	P. millim. cube.	Proportion to red corpuscles.
Before the opening of the abscess,	7,410	$\frac{1}{463}$
" " "	10,920	$\frac{1}{351}$
" " "	26,500	$\frac{1}{151}$
After the evacuatory punctures,	7,700	$\frac{1}{443}$
" " "	5,600	$\frac{1}{853}$

That is to say, the number of white blood corpuscles increased during the period of the formation of the abscess; whilst the pus once evacuated the white blood corpuscles decreased towards their normal number.

In a case of chronic abscesses discharging slowly, the enumeration of the white corpuscles made at several times showed that they varied between—

Number p. millim. cube,	11,200	and	17,500
Proportion to red corpuscles,	$\frac{1}{337}$	and	$\frac{1}{185}$

The increase of white blood corpuscles then takes place in cases of cold abscess as in cases of wound or acute abscess; which leads M. Malassez to suppose that this increase is little influenced by traumatic or suppurative fever, and that it is above all connected with the simple presence of pus in the organism.

M. Malassez, from those and other experiments, signalizes the constant coexistence of two conditions.

On the one hand, given suppuration in any part of the economy, there is, on the other hand, an increase of the white corpuscles of the blood.

The leukemia appears when the pus is produced, and disappears when it is evacuated, in such a manner that one might admit a *leukemia of suppuration.*

M. Malassez has also published a series of enumerations made in the blood of persons suffering from cancer and phthisis, for the details of which we refer to the original papers.

As regards cancer the general result is that, admitting the normal number of red corpuscles to be 4,500,000 in the male and 3,500,000 in the female (a small estimate), and comparing these figures with those found in the patients whose blood he examined, it was found that the corpuscles become 1·1 to 1·8 times less numerous in male patients and 1·1 to 2·2 times in female patients; that is to say, the hypoglobulie was nearly the same in both sexes.

It was further found that the hypoglobulie was more intense in proportion as the patients were more aged, and it progresses with the progress of the disease.

It seemed to M. Malassez as if the red corpuscles were less coloured than in health.

In phthisis, the results of 14 enumerations were as follows:—

	Men.	Women.
Maxima,	4,480,000	3,900,000
Minima,	2,560,000	980,000

Which shows that in the phthisical the red corpuscles in the blood can vary from the normal to a very small number, almost the fourth of the number in health.

In the first stage of phthisis the number was increased by treatment.—*Bulletins de la Société Anatomique de Paris.*

M. Malassez, whose description of the fungoid spores found in alopecia areata was summarised in a previous number of this Journal, has also found spores in the epidermic scales of ordinary pityriasis capitis. In the résumé of a paper entitled *Note sur le Champignon du Pityriasis Simple*, he remarks—

1st, In the general cases of simple pityriasis which I have examined, I have always found microscopic fungi.

2d, These fungi are composed solely of spores; they possess no tubes of mycelium.

The spores are in general ovoid and budding, rarely spherical, and are distinguishable from those of alopecia areata, which are usually spherical and are larger, and from those of other known fungi in that the latter have tubes of mycelium.

3d, The fungi of pityriasis are found in the horny layer of the epidermis; they penetrate into the follicles, but without reaching to the level of the orifices of the sebaceous glands. They are usually very abundant, but their abundance is not in proportion to the intensity of the pityriasis. They disappear when the pityriasis is cured.

4th, They seem to play in the pathology of pityriasis the same rôle as that of other parasites in the cutaneous affections considered to be of a parasitic nature.

5th, According to this view, the local treatment of simple pityriasis should be essentially anti-parasitic. This mode of treatment has been tried, and when well carried out has succeeded perfectly.—*Archives de Physiologie,* Juillet-Septembre 1874.

ABORTIVE TREATMENT OF GONORRHŒA.—Dr Godon publishes 35 cases of gonorrhœa successfully treated by the injection of clay-earth. "The clay is mixed with water to a consistency sufficient to allow it to pass through an ordinary rubber-bulb urethral syringe, by which means it is injected into the patient's urethra in the ordinary manner."

All the cases by this treatment got well under a week's time! The following note is appended to the paper:—"*Note.*—For a full description of the clay-earth, its *modus operandi*, and a number of interesting cases, I would refer the reader to Dr Hewson's elaborate and carefully-written work, and to my paper in the *Western Lancet*, No. 8, and also the *Medical Record* of New York for 16th March 1874."—*American Journal of Syphilography*, October 1874.

CHLORAL HYDRATE IN THE TREATMENT OF THE SO-CALLED DRY CHRONIC CATARRH OF THE MIDDLE EAR.—Lucae recommends that from ten to thirty drops of a mild solution be introduced into the middle ear by means of the Eustachian catheter and air-douche. A severe but short reaction generally follows. In the best cases, an improvement of the hearing is noted after the second application. If no improvement is seen after the twelfth application, we may expect none from the use of this remedy. In ten per cent. of all cases the improvement was marked; in twenty-five per cent. slight; and in sixty-four per cent. no improvement occurred.—*A. Lucae, Ueber locale Anwendung des Chloral Hydrats beim sogenannten trockenen Mittelohrcatarrh; Berlin Klin. Wochenschrift*, No. 41, 1872.

IN VARIOLA, the deep structures of the ear are not affected. The pustules of variola may appear upon the external meatus and the pharyngeal opening of the Eustachian tubes; the specific changes do not occur in the deeper parts of the structures already mentioned, nor in the cavity of the tympanum. These results are in accord with those gained by Dr Wendt of Leipsic. In one case Wendt found the labyrinth strongly hyperæmic. The last-named part has rarely been investigated in cases of variola.—*Ogston, Der Zustand der Gehörorgans bei Variola; Archiv. für Ohrenheilkunde*, Band vi.

Ruptur des Trommellfells bei einem Erhängten.—The rupture is described as ragged, and running from the end of the handle of the hammer downwards. The edges were turned outwards; there was no blood, nor any other fluid in the middle ear.—*Wendt, loc. cit.*

THE RESULT OF A SERIES OF EXPERIMENTS performed by Vulpian upon the chorda tympani show that the nerve contains centripetal fibres, which serve as a means of communication for excito-motory irritation, destined to act on the sub-lingual gland by reflex action. It contains, then, both centripetal and centrifugal fibres, which convey reflex excitation to the gland. Vulpian shows by his experiments that the fibres of the chorda tympani which accompany the lingual nerve in its distribution to the tongue, have the same influence on the vessels of that organ—*i.e.*, direct dilating influence—which the fibres of the chorda tympani possess over the vessels of the submaxillary gland, as already proved by Claud Bernard. He concludes his paper by remarking that the chorda tympani has little sensibility. Bonnafont, at a meeting of the Académie de Médecine, 16th July 1872, demonstrated that the phenomena of taste, etc., produced by irritation of the chorda tympani, and considered by Duchesne and Philipeaux to be of importance in the diagnosis of the degree of sensibility of the auditory nerve, possessed no real clinical value, and can never be used to determine the degree of sensibility of the auditory nerve, since there is no anatomical connexion between the two.—Vulpian, Function of the Chorda Tympani, *Gazette Médicale de Paris*, 15th Feb. 1873; Bonnafont in the *Mouvement Médicale*, 10th August 1872.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario College of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

Antagonism of Medicines.—The pages of the *British Medical Journal* have lately been occupied by the extremely interesting reports of the committee, appointed by the British Medical Association, to investigate the antagonism of medicines. The following are the general conclusions arrived at:—

I. *As to the Antagonism between Strychnia and Chloral Hydrate.*

In this investigation, 114 experiments were performed.

1. After a fatal dose of strychnia, life may be saved by bringing the animal under the influence of chloral hydrate.

2. Chloral hydrate is more likely to save life after a fatal dose of strychnia, than strychnia is to save life after a fatal dose of chloral hydrate.

3. After a dose of strychnia has produced severe tetanic convulsions, these convulsions may be much reduced, both in force and frequency, by the use of chloral hydrate, and consequently much suffering saved.

4. The extent of physiological antagonism between the two substances is so far limited, that (1) a very large fatal dose of strychnia may kill before the chloral hydrate has had time time to act; or (2) the dose of chloral hydrate must be so large in such a case to antagonize the fatal dose of strychnia, that there is danger of death from the effects of the chloral hydrate.

5. Chloral hydrate mitigates the effects of a fatal dose of strych-

nia by depressing the excess of reflex activity excited by that substance; while strychnia may mitigate the effects of a fatal dose of chloral hydrate by rousing the activity of the spinal cord, but it does not appear to be capable of removing the coma produced by the action of chloral hydrate on the brain.

II. *As to the Antagonism between Sulphate of Atropia and Calabar Bean.*

In this investigation, 114 experiments were performed.

1. Sulphate of atropia antagonizes to a slight extent the fatal action of extract of Calabar bean.

2. The area of antagonism is more limited than even Dr Fraser has indicated in his paper on the subject.

III. *As to the Antagonism between Hydrate of Chloral and Calabar Bean.*

In this investigation, 31 experiments were performed.

1. Hydrate of chloral modifies, to a great extent, the action of a fatal dose of extract of Calabar bean, mitigating symptoms and prolonging life.

2. Hydrate of chloral, in some cases, saves life from a fatal dose of extract of Calabar bean.

3. If hydrate of chloral be given before extract of Calabar bean, so that the animal is deeply under the influence of hydrate of chloral before it receives the extract of Calabar bean, the symptoms produced by the latter are much modified, and life is saved from the effects of what would otherwise be a fatal dose.

4. Chloral hydrate is of little service as an antagonist of extract of Calabar bean, if given some time after the latter. If the symptoms of the action of Calabar bean be in full operation, it will not save life, however it may modify the symptoms.

5. The antagonism is limited—

(*a.*) *By the amount of dose of the Calabar bean;* more than a minimum fatal dose of extract of Calabar bean destroying life, notwithstanding the administration of chloral hydrate.

(*b.*) *By the interval of time between the administration of the two substances.* There is a great probability of saving life in those instances in which the animal is under the influence of chloral hydrate before the subcutaneous injection of the Calabar bean; there is less probability when both substances are given simultaneously; there is still less if the chloral hydrate be given from five to eight minutes after the extract of Calabar bean; and no chance at all if the chloral hydrate be given eight minutes after a fatal dose of extract of Calabar bean.

6. Even in cases in which a fatal result follows the action of the two substances, the physiological effects of extract of Calabar bean are considerably modified by those of chloral hydrate.

IV. *As to the Antagonism between Hydrochlorate and Meconate of Morphia and Calabar Bean.*

In this investigation, 40 experiments were performed.

Hydrochlorate and meconate of morphia in no way antagonize extract of Calabar bean.

V. *As to the Antagonism between Sulphate of Atropia and Meconate of Morphia.*

In this investigation, 81 experiments were performed on rabbits and dogs.

a. In Rabbits.

1. Sulphate of atropia is physiologically antagonistic to meconate of morphia within a limited area.
2. Meconate of morphia does not act beneficially after a large dose of sulphate of atropia, for in those cases the tendency to death is greater than if a larger dose of either substance had been given alone.
3. Meconate of morphia is not specifically antagonistic to the action of sulphate of atropia on the vaso-inhibitory nerves of the heart.
4. The beneficial action of sulphate of atropia in cases of poisoning by meconate of morphia is probably attributable to the action which the former substance possesses of contracting the blood-vessels, thus diminishing the tendency to cerebral and spinal congestion produced by salts of morphia.

b. In Dogs.

Sulphate of atropia modifies the physiological action of meconate of morphia, and may even save life after a fatal dose of the latter. The limit, however, is so narrow as to be of no practical service.

VI. *As to the Antagonism between Tea, Coffee, Theine, Caffeine, Guaranine, on the one hand, and Meconate of Morphia on the other.*

In this investigation, 117 experiments were performed.

1. Theine is antagonistic to meconate of morphia, inasmuch as the action of the one substance modifies that of the other, and may even save life from a fatal dose of either substance.
2. Meconate of morphia delayed the appearance of the convulsions characteristic of the action of theine; but, on the other hand, theine, if given in large doses, did not affect to a marked degree the action of meconate of morphia, because symptoms of poisoning by theine were soon manifested.
3. Further experiments on cats showed that (*a*) while a cat may recover from the effects of a dose of 1¾ grains of meconate of morphia given alone, it will not recover from the effects of a dose of 2 grains, even although the effects of the latter dose are modified

by those following the introduction of 4 or 5 grains of theine; (*b*) that in three cases the animals recovered from the effects of $1\frac{7}{8}$ grains of meconate of morphia, and 4 to 5 grains of theine, while they died when the same dose of meconate of morphia was administered eight days after; (*c*) that when the dose of theine was increased beyond 5 grains, the animals invariably died, apparently from the effects of the theine.

4. Experiments on rabbits as to the antagonism between meconate of morphia and theine were found to be unsatisfactory as regards the purposes of this inquiry, because both drugs produce epileptiform convulsions in these animals.

5. The results obtained in investigating the action of caffeine and guaranine as antagonists to meconate of morphia, were similar to those observed with reference to theine.

6. Experiments were made on dogs to ascertain the effects of strong infusion of tea and decoctions of coffee as antagonists to meconate of morphia. These were unsatisfactory, chiefly because the tea or coffee was usually vomited so soon as to prevent the possibility of the exercise of any physiological antagonism. At the same time it was observed, in several instances, that the administration of tea or coffee so excited the animals as to prevent them from falling into stupor or coma after a dose of meconate of morphia, which would have produced this effect had the tea or coffee not been given.

VII. *As to the Antagonism between Extract of Calabar Bean and Strychnine.*

In this investigation, 30 experiments were performed.

Although the symptoms produced by either substance were modified considerably by the action of the other, there was no instance of recovery from a fatal dose.

VIII. *As to the Antagonism between Bromal Hydrate and Atropia.*

In this investigation, 36 experiments were performed.

1. There is a distinct physiological antagonism between bromal hydrate and atropine.

2. After a fatal dose of bromal hydrate, the introduction of atropia arrests excessive secretion from the salivary glands and mucous surfaces of the lungs, and thus obviates the tendency to death from asphyxia, caused by the accumulation of fluids in the air-passages. Atropia also causes contraction of the bloodvessels, and thus antagonizes the action of bromal hydrate, which causes dilatation of these vessels by paralysis of the sympathetic nerve.

3. While atropia may save life after a fatal dose of bromal hydrate, the converse apparently does not hold good, as we have never succeeded in saving life after a fatal dose of atropia by the subsequent injection of bromal hydrate.

ACTION OF NITRITE OF AMYL ON THE TEMPERATURE OF THE BODY.—Dr A. Ladendorf (*Berliner Klinische Wochenschrift*, No. 43) finds from many observations that the temperature always rises. The smallest maximal elevation noticed was 0·2° F., and the highest was 3·38° F. The mean rise was 0·7°.

NITRITE OF AMYL IN CARDIALGIA.—In all cases of cardialgia uncomplicated with gastric ulcer, Fuckel recommends the inhalation of a few drops of nitrite of amyl. In the neuralgic disorders accompanying menstruation, and in a case of rheumatic tetanus, he has also had satisfactory results with this agent.—*Deutsches Archiv für Klin. Med.*, 1874.

THE THERAPEUTIC ACTION OF IPECACUANHA.—M. C. A. Polichronie has recently published a work on the above subject. The following are his conclusions:—1. Emetine is the true active principle of ipecacuanha; 2. In dysentery, as in diarrhœa, ipecacuanha administered as an enema produces the same effects as when it is given by the mouth; 3. Ipecacuanha, as an enema, is one of the best treatments which can be employed in infantile cholera; 4. In the diarrhœa and sweating of phthisis, enemata of ipecacuanha give the best results.—*L'Union Médicale*, 26th Nov. 1874.

LOCAL ANÆSTHESIA BY SULPHIDE OF CARBON.—In the *Gaz. Méd. de l'Algeria*, Dr Charles Bernard relates several cases in which sulphide of carbon was employed to produce local anæsthesia. In one case six grammes, poured drop by drop on the part, and made to evaporate quickly, acted efficiently; and in another case, ten grammes applied by a spray apparatus enabled the operator to make six deep incisions into a large carbuncle without inflicting pain.—*New Remedies*, July.

Part Fifth.

MEDICAL NEWS.

NAVAL MEDICAL DEPARTMENT.—We congratulate our naval brethren, and the profession generally, on the Order in Council of the 4th February. All honour and thanks to the present Board of Admiralty, especially to its enlightened First Lord, and to the Medical Director-General, for so large a measure of justice to the Medical Department.

For more than ten years this School has practically uttered its emphatic protest against the disadvantages under which its brethren afloat have suffered, and for the removal of which they so long laboured in vain. But, now, we have great pleasure in cordially recommending the Navy, as perhaps the best field under Government, to those of our Students who are desirous of service under the Crown. The pay is good from the first, the rank and position of all grades cannot be found fault with, and every one has a fair prospect of advancement according to merit, and of retirement when preferred.

EDINBURGH UNIVERSITY COURT.—The University Court met 25th January. Present—Principal Sir Alexander Grant, Bart., in the chair; Lord Curriehill, the Lord Provost, Mr Macknight, Mr Campbell Swinton, the Rev. Dr Lindsay Alexander, and Sir Robert Christison, Bart. It was reported that no appeals had been lodged with reference to the register of the General Council for 1875. On consideration of additional information received from the Senatus, the arrangements for conducting the classes of Divinity and Pathology during the absence of the Professors from illness, were approved. The following appointments were made of additional Examiners in Medicine, under the regulations approved by Her Majesty, by Order in Council of the 6th August last, with power of annual reappointment by the Court for three years:—John Chiene, M.D. Edin., Lecturer on Surgery, Edinburgh, and formerly Demonstator of Anatomy in the University, to be Examiner on Anatomy; Angus Macdonald, M.D. Edin., Lecturer on Midwifery, to be Examiner on Midwifery; and James Dunsmure, M.D. Edin., formerly Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery, Edinburgh, to be Examiner on Clinical Surgery. With power of annual reappointment by the Court for four years:—Hugh Cleghorn, M.D. Edin., Stravithy, St Andrews, formerly Professor of Botany and Botanist to Government, Madras, to be Examiner on Botany; David Ferrier, M.D. Edin., Professor of Forensic Medicine, King's College, London, to be Examiner on Medical Jurisprudence. With power of annual reappointment by the Court for five years:—James Dewar, F.R.S.E., Lecturer on Chemistry, Edinburgh, to be Examiner on Chemistry; Dyce Duckworth, M.D. Edin., Assistant Physician to St Bartholomew's Hospital, London, to be Examiner in Practice of Physic; and William M'Intosh, M.D. Edin., Murthly, author of various works on Zoology, to be Examiner on Natural History. With power of annual reappointment by the Court for six years:—Joseph F. Payne, M.B. Oxon., B.Sc. Lond., Assistant Physician to, and late Demonstrator of Morbid Anatomy at, St Thomas's Hospital, London, to be Examiner on Pathology; and Thomas R. Fraser, M.D. Edin., F.R.S.E., Medical Officer of Health for Mid-Cheshire, formerly Lecturer on Materia Medica in Edinburgh, to be Examiner on Materia Medica. With power of annual reappointment by the Court for seven years:—James Dunsmure, M.D., to be Examiner on Surgery; D. R. Haldane, M.D. Edin., Lecturer on Practice of Physic and on Clinical Medicine, Edinburgh, to be Examiner on Clinical Medicine; and Arthur Gamgee, M.D.

Edin., F.R.SS. London and Edinburgh, Professor of Practical Physiology in Owens College, Manchester, to be Examiner on Institutes of Medicine. An additional report by the committee on the University accounts for 1873-74 was submitted and adopted, and ordered to be sent to the Senatus for their information. A report by the committee on the class returns 1873-74 was submitted and adopted, and ordered to be sent to the Senatus for their information. —*Communicated by direction of the University Court.* J. CHRISTISON, W.S., *Secretary.*

ROYAL MEDICAL SOCIETY OF EDINBURGH, 1874-75.—The following gentlemen have been elected Presidents for the year:—Robert Saundby, M.B.; William Garton, M.R.C.S. Eng. L.S.A.; M. R. Simpson; and R. A. Gibbons. M.B., C.M., M.R.C.S. Eng.

The following is a list of ARMY MEDICAL CANDIDATES who were successful at both the London and Netley Examinations in August 1874, and who passed through a course of instruction at the Army Medical School, Netley:—

	No. of Marks.		No. of Marks.
1. Harrison, C. E.	5645	9. Gardner, R. H.	3680
2. Wellings, B. W.	5175	10. M'Gann, J.	3678
3. Forrester, J. S.	4095	11. Powell, J.	3660
4. Smith, R.	4035	12. Carter, S. H.	3490
5. Trevor, F. W.	3955	13. May, W. A.	3447
6. Mullane, T.	3948	14. Bourke, G. D.	3427
7. Scott, H.	3785	15. Gardner, H. G.	3367
8. Campbell, W.	3755	16. Hoysted, J.	2996

LIST OF INDIAN MEDICAL CANDIDATES who were successful at both the London and Netley Examinations in August last, having passed through a course of instruction at the Army Medical School, Netley.

	No. of Marks.		No. of Marks.
1. Stoker, R. N.	5290	8. O'Keefe, J. L.	4145
2. Bomford, G.	5222	9. Adams, A. F.	4100
3. Barclay, A.	5206	10. Hume, T.	4087
4. O'Hara, W.	4785	11. Parakh, D. N.	3887
5. Tootell, E	4780	12. Oliver, J. P.	3795
6. Gray, H. A. C.	4410	13. Lucas, J. C.	3732
7. M'Cartie, C. J.	4348	14. Sweetnam, M.	3515

OBITUARY.

THE LATE WILLIAM DUMBRECK, M.D., F.R.C.S.E.—This much esteemed member of the profession died at his residence in Albany Street, on the 2d of January 1875. Many will miss his genial presence in their midst; for, besides being the urbane courteous gentleman, which his aspect and demeanour betokened, he was always reliable, reasonable, and sympathetic, without egotism, without undue self-consciousness, without *corners* of any kind. A capable man, of sound medical and surgical attainments, he was specially accomplished as a student of natural history in its various departments. This was made apparent by the very efficient manner in which he discharged the duties of an Examiner in the Royal College of Surgeons, in the Veterinary College, and latterly, in the University of Edinburgh, where no topic of examination seemed to come amiss to him. Dr Dumbreck may be said to have died in harness; for although in his seventy-fifth year and far from strong, he continued to perform his duties, many of them arduous, with much courage and self-abnegation, until within five weeks of his death, when he was prostrated by severe and complicated pulmonary disease. This little sketch would be very incomplete, if we neglected to remind his friends that he was an enthusiastic lover of music; an adept in the "gentle art," which he practised whenever he could get an opportunity; and, above all, a humble and sincere Christian.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

EDINBURGH, 19*th January* 1875.

MY DEAR SIR,—I send you the enclosed letters, addressed to the Editor of the *Glasgow Herald*, on a subject which seems to me to be important in the highest possible sense as a sanitary question, and I think it might not be amiss to connect it with the case of Edinburgh, by pointing out that, however much better off we may be, we are in great risk from the same causes. I took some pains, when Provost Chambers got his Act, to express publicly that, if we were to be assessed heavily for clearing away houses which never ought to have been built, it followed that we should submit to such restrictions on the right of building over our own properties as are obviously required for the purpose of preventing private gain and private cupidity—which are always at work—from creating a necessity some years hence for again thinning the city of injurious buildings. Notwithstanding all my efforts, the Act was got without any such clause; and when I wrote about it to Mr Chambers, he told me that such a clause would, if insisted on, have been fatal to the Act itself.

One needs only to take a glance at the New Town of Edinburgh in order to discover that warehouses, shops, and churches are often placed in positions in which they necessarily shut out air and light from dwelling-houses, and where it is not easily possible to imagine that those who erected them were ignorant of the obvious injury done to the neighbourhood. It is hard to be called on to submit to exactions for the remedy of evils which ought not to have existed; but it is still harder to do so without any remedy against the same evils being repeated.

I could not help casting these few thoughts together, of which you may, or may not, make use as you think best.

JOHN GAIRDNER.

P.S.—If it be the wisdom of our legislators that we should go on inflicting serious evils on our neighbours according to law, and should then call on these neighbours to submit to be assessed for the remedy of those evils, the sooner we convince such legislators that their wisdom is foolishness, the better it will be for all of us.

J. G.

CHEMICAL VAPOURS, FOG, AND THE DEATH-RATE.

GLASGOW, *5th January* 1875.

SIR,—The experience of the last three weeks ought to do something towards settling, in the minds of most of us, a question on which I have heard various opinions expressed, and on which, perhaps, it is not inconvenient for some more or less influential persons to maintain doubts and difficulties as a part of their creed. Every one probably has heard it affirmed that manufacturing vapours are injurious to health; and the contrary paradox, that, although to a certain extent unpleasant and irritating, they are *not* demonstrably injurious —nay, perhaps, may even be useful as disinfectants—has been often maintained. When dealing with this subject in 1869, under the pressure of what was then considered an extremely high death-rate—though very far short of that which has been lately announced to us—I came to the conclusion, after an elaborate investigation, that manufacturing vapours had probably some considerable influence in increasing the local death-rate from pulmonary causes during northerly and easterly winds; and in particular I pointed out that the purely climatic influences to which the Scottish Registrar-General had attributed the excess of mortality in 1869, had been almost inappreciable in their results in Aberdeen and Perth, and but slight in Edinburgh, as compared with Glasgow and Greenock, although the first three cities are notoriously much more exposed to the merely chilling effects of those winds than the two last. I further remarked that "among the *constant* sources of high mortality in Glasgow, diseases of the lungs are always prominent, . . . and that both in Edinburgh and Aberdeen we have examples of cities far more exposed to the influence of (northerly and easterly winds) in their *pure* state than Glasgow, and in which, nevertheless, the pulmonary death-rate is not only lower at present, but is persistently lower, than with us." These conclusions were expressed with all the care and scrupulous reserve imposed by the responsibilities of an official position; but is it not rather a striking commentary upon them that now, in the midst of choking fogs such as every Glasgow citizen has personally experienced of late, we find the death-rate running up to over 60 in the thousand, far in advance of previous seasons in Glasgow itself, and far in advance also of every other city in the empire? To say that manufacturing vapours are the sole, or even the chief, source of the mortality of Glasgow, would probably be an error. But to say that manufacturing vapours, when condensed by the state of the atmosphere into a fog that brought Egyptian darkness into our houses at noonday, formed a very serious aggravation of the causes of ill-health derived from the atmosphere alone, is nothing more than an obvious truth, to which the terrible figures of the death-rate announced by you this morning correspond in a remarkable manner.

Now, if it be a fact that Glasgow and Greenock have habitually, or, indeed, all but constantly, the highest *pulmonary* death-rate in Scotland, is it not extremely probable that the vapours which, when condensed into a fog, bring about such death-rates as we have had lately, have something also to do with the *constant* excess of mortality from pulmonary causes in these cities? Dr

Russell will probably be able to tell us something more about this by-and-by, but, in the meantime, while the memory of the "great fog" is still searching and keen among us, and its effects perhaps still lingering about our throats, I trust our good citizens and their rulers will alike determine to do what can be done to prevent such "killing times" in future.—I am, etc.,

W. T. GAIRDNER.

THE DEATH-RATE IN GLASGOW—THE DEBATE AT THE POLICE BOARD.

GLASGOW, *13th January* 1875.

SIR,—I did not intend to trouble you again with any observations on the unfavourable state of the public health in Glasgow, not doubting that Dr Russell will sooner or later draw attention to the lessons to be learned from it with the aid of all the evidence which the detailed statistics of the sanitary office offer on the subject. But the late debate at the Board of Police gives the opportunity of making a few remarks which, as coming from an ex-official, may perhaps have some influence for good.

Two of the speakers, Mr W. R. W. Smith and the Lord Dean of Guild, appear to me to have given a certain amount of prominence to what I should consider the chief lesson to be learned from the late inordinate death-rate. Glasgow is a permanently unhealthy town, because it is a too concentrated town; and concentration, in the case of Glasgow, means not only increased risk from epidemics, and all other diseases depending upon human exhalations, but also greatly increased risk, in such weather as we have had lately,'from condensed vapours arising from her numerous manufactories. When severe cold brings all these noxious exhalations into the state of greatest density and noisomeness, and when the poor have to take refuge in their overcrowded and comfortless homes from the chilling and yet more comfortless atmosphere of the streets, then the evils that happen to all of us, in such seasons happen to them in a more aggravated degree. The measures devised by the Relief Committee, and explained by the Lord Provost on Monday, are admirable so far as they go; but if we are ever to look forward to a permanent lowering the death-rate in Glasgow, it must be by means tending to diminish the risk of such a state of the atmosphere as we all experienced only a fortnight ago. The lesson of our inordinate death-rate during the last few weeks will not have been read, unless we remember that the causes which were in operation then are always in operation, more or less, and that the stifling fogs of Christmas were only concentrated samples of an atmosphere that surrounds us continually, both summer and winter. The habitual *pulmonary* death-rate of Glasgow, *nearly twice as great as that of Edinburgh, and perhaps three times as great as that of yet more favoured places*, is the scientifically exact record of the permanently polluted state of our atmosphere, added to the other causes of mortality always existing in crowded centres of population. A death-rate of over 60 in an irrespirable fog, is simply an exaggeration of the causes which produce a permanent death-rate of 30, and give to Glasgow its notoriously bad eminence in this respect. Hence the practical lesson to be derived from the late inordinate rate of mortality, is the necessity of diminishing the permanent rate; and this, I apprehend, can only be done by *the gradual dispersion of the town over a larger area*, thereby diminishing at once the concentration of noxious vapours, and the dangerously excessive density of the resident population.

The operations of the Improvement Trust have been very successful in clearing out some of the most densely peopled spots in the centre of the city, with great benefit alike to the health and morals of their immediate neighbourhood, as Bailie Morrison so clearly showed in his paper read at the Social Science Congress. But the persons displaced by the City Improvement Act have not in all probability found their way to any extent into the suburbs—in other words, the Improvement Act, although greatly improving the spots which it affects, has not to any considerable extent *decentralized Glasgow*. And this is what we must aim at doing—gradually, of course—if we are ever to have a purer atmosphere, a less overcrowded population, and a lower death-rate. We must spread our manufactories, as well as the people who work in them, over a much larger area than at present, and instead of living nearly 100 persons on an acre overhead (open spaces included), we must in every way encourage the removal both of our people and of their workshops into the open, and sternly prohibit the establishment of new sources of nuisance and new inducements to overcrowding in quarters which are already far too densely built. The example of Mr Walter Macfarlane in this respect ought to be the model for the Glasgow of the future; although, of course, it is impossible to deal with all established interests in a like way. All new manufactories should be required to provide not only for the space to be occupied by the works carried on, but also in a certain measure for the population which they cause to spring up; and as this would involve a question of the expense of ground on a considerable scale, it would probably have the effect of guarding our crowded centres from further intrusion. Furthermore, the sources of the pollution of the air, water, sewers, etc., inherent in the processes of manufacture proposed to be established in our midst, should be rigidly dealt with *in advance; i. e.*, no public work ought to be established without details submitted and securities given on these points. And, lastly, our present Smoke Acts ought to be far more stringently enforced than they are among us; and in this respect we should look to the example of London, where the Home Secretary acts as public prosecutor, with the best effect as regards the permanent diminution of noxious vapours.

With the remarks of the Dean of Guild, as regards the inexpediency of four-storey tenements, as a rule, for working men's houses, I entirely sympathize, and I heartily wish that powers were given to his Court, or some other authority, to restrain the erection of huge blocks of such tenements, without adequate open spaces, and all sub-let in houses of one or two apartments. But the necessity for this system of building has arisen in a great measure from the too great massing together of our industrial establishments; and if laws or inducements could be brought to bear on the latter evil, it would be much more easy to deal with the former.—I am, etc., W. T. GAIRDNER.

To the Editor of the Edinburgh Medical Journal.

EDINBURGH, 16*th February* 1875.

SIR,—In a paper by Mr Annandale in the Journal for this month, on a new form of club-foot boot, he alludes to the methods generally in use for attaching the foot to the foot-piece of the boot—1*st*, By lacing it into a boot attached to the foot-piece; 2*d*, By fixing it to the foot-piece by a broad strap passing over the instep. As I have found both of these plans inefficient in aggravated cases of talipes equinus, I beg to recommend a plan which is most efficient, and which I do not find noticed in any of the text-books on surgery, namely, to apply a broad strap of strong sticking-plaster on either side of the limb from the knee to the ankle, with tapes attached, which are passed through slits at the sides of the heel-piece, and then tied, drawing the heel firmly down to the foot-piece. This is an adaptation to the treatment of club-foot of the method by which the extension apparatus is attached to the limb in the treatment of fractures. A single trial will at once render its advantages apparent. By it I have overcome what was to me *the* great difficulty in the treatment of club-foot.—I am, yours etc., JOHN CHIENE.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Means of Ascertaining the Length of Gestation by Measurements of the Fœtus and Gravid Uterus during the Second Period of Pregnancy.* By VASSILY SUTUGIN, M.D., Physician to the Court of His Imperial Highness the Tsarevitch, Physician to the Maternity Hospital of the St Petersburg Foundling House. Translated from the Author's Manuscript by GEORGE L. CARRICK, M.D.

(*Communicated to the Obstetrical Society of Edinburgh by* DR MATTHEWS DUNCAN, *13th January*.)

EVERY objective symptom which serves to point out the period gestation has arrived at, is of great value, not only at the patient's bedside, but also from a medico-legal point of view. In spite, however, of the interest attached to such a subject, we have very few exact observations in regard to it; although in text-books on midwifery, reference is made to the size of the fœtus during successive months of pregnancy, and also to the position of the fundus uteri in its relation to the symphysis pubis and the umbilicus. The size of the fœtus, prior to Ahlfeld's essay,[1] was determined only after birth, while the height of the fundus uteri was measured either by the fingers or by a tape measure,—a procedure which, considering the difference of height of the umbilicus in different women, furnished extremely contradictory results. The determining, by means of a tape measure, of the distance between the symphysis pubis and the fundus uteri, even in cases where the children are of the same size, is also fallacious, partly owing to the unequal development of adipose tissue under the skin in different females, partly to the varying convexity of the uterus. Our conclusions will of course be still more contradictory when the children differ in size and position. Indeed fœtuses differ from each other both in length and weight during almost every period of gestation.

Elsaesser,[2] and after him Hermann,[3] have already drawn from the materials at their disposal the following conclusions:—"That frequently children born during the earlier periods of pregnancy surpass in length those brought into the world at a later date. Therefore it is impossible to judge at what period of gestation the fœtus was born by the length of the latter. The age of the infant and its size, are far from standing in exact relation to each other; children arrived at their full time, and even those carried beyond it, may be born small in size and light in weight." "Neither the duration of pregnancy, nor the weight of the fœtus, nor its length, nor the relation between the upper and lower parts of the body, taken separately, can serve as indications of the age of the fœtus; the only trustworthy signs are the degree of its development, and proofs of its body having performed vital functions." While entirely agreeing with Elsaesser that there is no *single* certain sign of the fœtus having arrived at maturity, and that only all the signs taken together can demonstrate, with anything like exactitude, the age of the fœtus, we cannot accept such statements as final, without also taking into consideration the results arrived at by such observers as Hecker,[1] Schroeder,[2] and Ahlfeld, whose unanimous opinion it is that the length of the fœtus serves as the surest indication of its age. The above-mentioned authors' observations were carefully made, either by themselves or by trustworthy assistants; whereas Elsaesser says nothing of the method of investigation employed by himself. Perhaps the materials he made use of were collected by midwives or by persons as little accustomed to strict observation. Now, every practising physician is well aware of the worth of observations made by such people,—although in certain exceptional cases Elsaesser's conclusions seem quite correct. Hence only accurate clinical records are valuable, and opinions founded thereon can be refuted only by a series of observations similarly made. I regret that I have been able to collect only a small number of facts in regard to infants born before the full time; but with the view of verifying, after birth, the measurements of these children taken before they were born, I consider the bringing forward of even such small data as not out of place,—data which I hope to extend as opportunity offers. In estimating the maturity of the fœtus, the period of gestation as determined by the last catamenia, the corresponding size of the womb, and the general development of the new-born infant, were taken into account. As nearly all my patients were unable to define the day of conception, but only their last menses, I cannot consider myself as fortunate in my cases as Ahlfeld was in his.

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[1] Ahlfeld, Archiv für Gynaek., B. ii. p. 352-373, "Bestimmungen der Grösse und des Alters der Frucht."

[2] Elsaesser, Henke's Zeitschrift für Staadtsarzneikunde, 1858, Bd. 75 (245-294).

[3] Hermann, Monatsschrift für Geb. und Fr.-Kr., B. xii. pp. 394, 395.

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[1] Hecker und Buhl, Klinik der Geburtsk., Leipzig, 1861, 131–45; und Band ii. pp. 22–23.

[2] Schroeder, Geburtshülfe. Vierte Auflage. Bonn, 1874.

Table I.

Period of Pregnancy.	Weight of Child.	Length of Child.	Number of Cases.	Average Weight of Child.	Average Length.	Maximum Weight.	Maximum Length.	Number of Cases.	Minimum Weight.	Minimum Length.	Number of Cases.
Weeks.	Grm.	Ctm.		Grm.	Ctm.	Grm.	Ctm.		Grm.	Ctm.	
44	3520	51·5	1 Longer than 40 weeks.								
44	3200	48·5	1								
41·5	4300	54	1								
41	3145	45·75	1	3438·5	49·9	4300	54	1	3145	45·75	1
	3100	49	1								
	3575	50·75	1								
	3230	50	1								
40			108	3412	48·4	5250	55	1	2449	43	1
39	2922	46	1								
	2390	44	1								
	3020	49	1								
	3230	46	1								
	3800	49·5	1								
	4230	49	1	3016	47·5	4230	52	1	2200	43·5	1
	2200	46·5	1								
	2535	45·5	1								
	2725	48·5	2								
	3170	49·5	1								
	2350	43·5	1								
	3100	49	1								
	3300	52	1								
	3132	48·5	1								
	3590	48·5	1								
	3545	48	1								
	2800	46·5	1								
	2500	46	1								
38	3430	45	1								
	3080	46	1								
	2950	47·5	2	2897·3	46·6	3600	48·5	1	2350	44·25	1
	3600	47	1								
	2350	45	1								
	2650	46	1								
	2950	48·5	1								
	2825	48·5	2								
	2559	44·25	2								
37	2650	46	1								
	2720	48	1								
	3035	46	2								
	2800	48·5	1	2770	46·5	3035	48·5	1	2250	45	1
	2900	46·5	1								
	2250	45	1								
36	2920	47·75	2								
	2780	45·85	3								
	2500	46·25	2								
	2750·5	47·75	2								
	2420	46·5	1	2622	46·5	3097	49·5	1	2030	42·5	1
	2301	49·5	1								
	3097	47·5	1								

TABLE I.—*Continued.*

Period of Pregnancy.	Weight of Child.	Length of Child.	Number of Cases.	Average Weight of Child.	Average Length.	Maximum Weight.	Maximum Length.	Number of Cases.	Minimum Weight.	Minimum Length.	Number of Cases.
Weeks.	Grm.	Ctm.		Grm.	Ctm.	Grm.	Ctm.		Grm.	Ctm.	
36	2030	42·5	1								
	2200	44·5	1								
	2620	47	2								
35	2500		1	2700	46·25	2900	46·5	1	2500	46	1
	2900	46·5	1								
	2700	46	1								
34	2280	44·5	1	2440	45·3	2550	46	1	2280	44·5	1
	2490	45·5	1								
	2550	46	1								
33	2270	41									
32	1850	41·5	2	2064	43·1	2450	48	1	1850	41·5	2
	2085	42·25	2								
	2450	48	1								

In regard to the earlier births, there were only a few cases, and I have therefore omitted them. For children arrived at maturity, I shall simply give the average figures, as too many details would exhaust the reader; and, further, as such details are matters of secondary importance, in so far as this essay is concerned.

For the more complete support of my measurements of the fœtus, I shall cite the figures of Hecker, Ahlfeld, and Schroeder.

TABLE II.

	Period of Pregnancy.	Average Weight according to Hecker.	Average Weight according to Schroeder.	Average Weight according to Alhfeld.	Average Length according to Hecker.	Average Length according to Schroeder.	Average Length according to Alhfeld.
Months.	Weeks.	Grammes.	Grammes.	Grammes.	Ctm.	Ctm.	Ctm.
10	40	2450	2528	3168	45·47	46	50·5
	39			3321			50·6
	38			3016			49·9
	37			2878			48·3
9	36	1993	2240	2806	42·44	44·6	48·3
	35			2753			47·3
	34			2424			46·07
	33			2084			43·88
8	32	1609	1700	2017	39·41	41·3	43·4
	31			1972			43·7
	30			1868			42·0
	29			1576			39·6
7	28 27	1343		1142	35·38		36·3

On comparing Table II. with my own measurements of new-born children, it appears that my figures approach most closely to Ahlfeld's, although infants carried to the full time are shórter in proportion to their weight with us in St Petersburg than in Leipsic. Ahlfeld, as well as Hecker, have arrived at the conclusion that "the greater the length of the fœtus, the greater the probability of the head being large." The former gives us a table of the comparative length of the child, and of the breadth of its skull. The results obtained by myself bear a close resemblance to Ahlfeld's figures. I shall therefore confine myself to a citation of my own measurements, which will be found in Table III. In constructing the following Table, I have given measurements of the longer transverse diameter of the head (from the tuberosity of one parietal bone to that of the other), and of the shorter one (between the zygomatic processes). All these measurements were made by myself.

TABLE III.

On the Relation of the Length of the Fœtus to its Breadth.

Length of Child's Body.	Number of Measurements	Maximum Length of Biparietal Diameter.	Minimum Length of Bizygomatic Diameter.	Number of Cases.	Average Length of Biparietal Diameter.
Ctm.		Ctm.	Ctm.		Ctm.
55	1	10·8	8	1	...
54	1	10	8·75	1	...
53	3	9·5	8·3	3	...
52	9	9·2	8	9	...
51	13	9·1	7·6	...	...
50	15	9·03	7·8	...	...
49·5	10	9·0	7·9	28	8·9 to 7·7
49	18	8·8	7·6		
48·5	16	9·1	7·6	37	9 „ 7·6
48	21	8·9	7·6		
47·5	5	9·1	7·8	16	8·9 „ 7·7
47	11	8·8	7·5		
46·5	12	8·5	7·1	27	8·6 „ 7·1
46	15	8·7	7·2		
45·5	4	8·8	7·3	8	8·6 „ 7·3
45	4	8·5	7·4		
44·5	4	8·4	7·1	7	8·4 „ 7·3
44	3	8·5	7·5		
43·5	1	8·75	7·5	...	...
43	...	...	...	...	...
42·5	1	9	7·5	2	8·2 „ 7
42	1	7·5	6·5		
41·5	1	7·5	7	2	7·7 „ 7
41	1	8	7		

As will be seen from the above cited figures, there is no strict mathematical relation between the length of the child and the breadth of its head; but, in the majority of cases, the most correct conclusion is, that the longer infants have the broader heads. To ascertain the size of the head, however, it is not sufficient to measure the short diameter, but other measurements should be taken as well.

Thus, Pfannkuch found that the circumference of the fœtal head is equal to the sum-total of three diameters—the transverse, the antero-posterior, and the oblique[1]—and he therefore considered the circumference of the head as the expression of the degree of its development. On the basis of his measurements, he arrived at the conclusion that the weight and length of the body, and the size of the head, keep pace in growth until the child arrives at its medium weight, while afterwards, during the gradual increase in weight, the increase in length first lags behind, and then the increase in the size of the head. The more rapid development of the weight goes towards enlarging the circumference of the child's body. I have myself made 167 such measurements, and 50 more my friend Dr Stoll was so obliging as to make for me, so that my table consists of 217 measurements in all. In this table, live-born children, carried to the full time, and delivered without any extraneous aid, in cases of normally developed pelves, are taken into account. In order to strengthen the limited number of my observations, I shall take the liberty of also quoting Pfannkuch's figures.

TABLE IV.

The Relations of the Circumference of the Head and the Length of the Body to the Weight of the latter in Newly-Born Children.

Weight of Child in grammes.	Circumference of Head (my measurements) in centimetres.	Circumference of Head according to Pfannkuch (in centimetres).	For each kilogramme of weight a circumference (in centimetres) of	Length of Child in centimetres (my measurements).	Length of Child according to Pfannkuch (in centimetres).	For each kilogramme of weight a circumference (in centimetres) of	No. of Children measured by me (in centimetres).	No. of Children measured by Pfannkuch (in centimetres).
1500 to 2000	29·6	30·37	...	40·2	42·59	...	5	23
2000 ,, 2250	31·6	30·75	14·8	43·2	44·02	20·32	8	36
2250 ,, 2500	32·0	31·33	13·4	45·3	45·46	19·07	12	52
2500 ,, 2750	32·3	32·28	12·3	47·2	46·81	17·98	25	90
2750 ,, 3000	33·1	22·79	11·13	47·5	47·76	16·52	35	110
3000 ,, 3250	33·75	33·62	10·8	47·9	48·76	15·32	48	150
3250 ,, 3500	34·1	34·62	10·01	49·1	49·53	14·54	35	115
3500 ,, 3750	34·5	34·27	9·51	49·3	50·33	13·60	36	79
3750 ,, 4000	34·7	34·84	8·95	48·9	51·05	12·61	11	46
4000 ,, 4500	35·9	35·4	...	52·1	52·46	...	11	13

My measurements coincide with Pfannkuch's, viz., that, at the commencement of each kilogram of the child's weight, there is a circumference of the head of 14·8 centimetres, and a length of the body of 20·3 centimetres. Afterwards, however, when the child reaches a weight of 3¼ of a kilogram, there is a circumference of only 8·95 ctm., and a length of 12·61 ctm. for every kilogram. Consequently, Pfannkuch's above-cited conclusion is confirmed in so far as the average of the cases is concerned; but in many individual instances, the circumference of the head presents almost as great variations in regard to the weight as it does to the length of the body, so that for the present I am bound to agree with Hecker, Schroeder, and Ahlfeld, and regard the length of the child as the more certain indication of the degree of its development. After the infant's birth, there are within our reach a great many other signs which afford us the opportunity of judging of its development, and thus of the period of pregnancy at which labour had arrived. I have touched upon this question but slightly, as I intend passing over to the chief subject of this essay, viz., the possibility of defining the size of the fœtus during pregnancy. Ahlfeld, in making a newly-born infant assume the position it occupied *in utero* (*i.e.*, bending its head, and crossing its arms over its breast, while its bended knees were brought up towards the abdomen, so that it presented an egg-shaped outline) found that the length between the opposite poles of the oval form was exactly one half the length of the fœtus. In proof of the correctness of his statement, Ahlfeld brought forward a whole series of measurements (250 in all) of newly-born infants. In three cases in particular, these children were covered with vernix caseosa, and it was thus easy to place them in the position they had occupied before birth. As a further proof of the correctness of his conclusions, Ahlfeld measured the frozen body of a woman in the last stage of pregnancy (case published by Braune[1]), and found that the length of the oval position of the fœtus equalled one half its whole length. Ahlfeld further remarks that the bending of the child to make it assume the same position it had *in utero*, demands the use of considerable force. He measured a number of children *in utero*, and found his aphorism, that *the length of the child in the womb almost equals half its length when straightened out*, confirmed. In measuring the fœtus before birth, Ahlfeld proceeded as follows:—He placed the pregnant female on an even, and somewhat hard, bed, so that the pelvis might be on a level with the upper part of the body, made her draw her knees (which were held apart) up to the abdomen; after which, he firstly exactly defined the position of the fœtus, and then proceeded to measure its size. In normal presentations, *i.e.*, with the head or breech presenting, the highest point of the fundus uteri is defined by means of palpation and percussion; in very corpulent females, by percussion alone. The most superior point of the fundus uteri, where the head or breech is situated, generally presents one of the poles of the oval (the outline, as already mentioned, of the position of the fœtus), the other pole may be felt either outwardly through the abdominal walls, just above the inlet of the pelvis, or else directly through the vaginal pouch, which is effected much more easily, and with greater exactitude. After defining the upper pole of the oval, the point corresponding to it on the linea alba is marked off with ink. After ascertaining the position of the two poles of the fœtal "oval," the points of Baudelocq's pelvimeter are placed over them respectively, and the length of the child is thus measured. The thickness of the uterine walls is insignificant, as was shown by Braune's measurements on the dead body of a woman in the last stage of pregnancy.

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[1] Ueber die Koerperform der Neugeborenen; Arch. für Gyn., B. iv. pp. 297–310.

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[1] W. Braune, Die Lage des Uterus und Fœtus, Leipzig, 1872.

TABLE V.

The Average Length of the Fœtus during successive weeks of Pregnancy.

Week of Pregnancy.	Length of Fœtus.	Number of Cases.	Length of Child after Birth (my Measurements).	Length of Child before Birth according to Ahlfeld's Measurements.	Length of Child after Birth according to Ahlfeld's Measurements.
	Centimetres.		Centimetres.	Centimetres.	Centimetres.
40	24·4	99	48·4	25·6	50·5
39	24·4	77	47·5	25·6	50·6
38	23·9	70	46·6	24·9	49·9
37	23·4	49	46·5	24·1	48·3
36	23·3	40	46·5	23·9	48·3
35	23·1	26	47·5	22·5	47·3
34	22·7	19	45·3	23·0	46·07
33	22·7	13	...	22·3	43·88
32	21·9	9	43·1	21·5	43·4
31	21·9	8	...	21·7	43·7
30	19·8	7	...	20·9	42
29	20·0	4	38·0	20·2	39·6
28	20·0	2	38·5	19·4	40·4
27	19·0	2	...	18·9	36·3
26	19·0	1	35	17·7	...
25	...	...	...	18·3	...
24	18·0	1	...	13	...
23	18·0	1	...	...	...
22	17·0	1	...	...	...

In oblique presentations, Ahlfeld advises the straightening out of the fœtus's position, before proceeding to measurement. In cases where this would be difficult of accomplishment, or in transverse presentations, he counsels the direct definition of the most prominent parts of the oval, viz., the head and breech, and, placing the two extremities of the pelvimeter thereon, measures their distance from each other. In carrying out these measurements, Ahlfeld very wisely cautions us to endeavour to avoid exciting uterine contraction, which is very easily produced by the manipulation necessary for a correct examination. His advice is to make our measurements only when the uterus is at rest. In measuring the fœtus in utero, I followed Ahlfeld's above-mentioned rules, with this difference only, that I did not mark the situation of the upper part of the fundus uteri with china ink, but at once placed one extremity of the pelvimeter over its most superior point detectable by palpation, as I discovered that the spot marked on the skin used, during the movements of the fœtus, not unfrequently to change its position in regard to the lower portion of the oval. I have myself made 409 measurements, and these were taken during various periods of pregnancy, and from once to twice weekly. In the above table (No. V.) only those measurements are taken into account, where there was either a natural head presentation, or an oblique one in which the fœtus could be straightened out.

From an average of the figures brought forward, it will be observed that the measurements of the fœtus before birth, very closely approach the measurements taken after birth. To elucidate the matter still further, I have cited Ahlfeld's figures, from which it will be perceived that he had to deal with children greater in length than those that came under my notice, which entirely coincides with the measurements made of children in St Petersburg immediately after their birth. During the earlier months of pregnancy, Ahlfeld's measurements bear a close resemblance to mine. The measurement of the fœtus *in utero* can only be undertaken when the projecting parts of the child are easily felt, and when the womb keeps its solid contents fixed, *i.e.*, from the seventh month of pregnancy. The mistakes amounted to about 18 per cent. of the measurements hitherto made; the errors arising chiefly from an excess of liquor amnii, and a puny fœtus which floats freely in the fluid, and is not doubled up in the manner it would have been, with the ordinary amount of waters, in the womb.

Moreover, strict attention should be paid to the state of the uterus, *i.e.*, whether it is contracted, as the contractions of the womb produced by examination, lengthen the fœtus, according to my observations, some 4 centimetres during the last three weeks of pregnancy, and from ½ to 2 centimetres from the thirty-fifth to the thirty-seventh week of pregnancy. At the commencement of labour, the lengthening of the fœtus is still more marked. Ahlfeld found the difference as great as 5 centimetres sometimes. The contractions of the uterus which take place before actual labour commences, produce a change in the length of the fœtus, so that most of the errors in measurements arise from these measurements having been made from one to five days prior to parturition. In proof of this, I shall bring forward measurements made during labour pains, at the height of uterine contraction, and also in the intervals thereof.

TABLE VI.

Measurements of the Fœtus during Parturition.

Order of Measurements.	Length of Fœtus without Uterine Contractions.	Length of Fœtus during Uterine Contractions.	Length of Child after Birth.	Order of Measurements.	Length of Fœtus without Uterine Contractions.	Length of Fœtus during Uterine Contractions.	Length of Child after Birth.
	Ctm.	Ctm.	Ctm.		Ctm.	Ctm.	Ctm.
1	26	29	26·5	11	26	28	24·5
2	26	29	24·25	12	25·5	28	25
3	24	26	22	13–14	24·5	27	24·5
4	25·5	28	24	15	25	26·5	23·25
5	25·5	27·5	23·5	16	26	28	23·25
6	25·5	27	23	17	26	28	25
7	25	27·5	25·5	18	24	25	22
8	25·5	29	25	19	26·5	28	25
9	26·5	29·5	24	20	27	29	24·0
10	26	28	25				

Out of thirty similar observations, I found the greatest length of the fœtus to be 35 centimetres, *i.e.*, an increase of 1½ centimetres. Generally those measurements made during labour can alone be taken into account in which the womb remains but very slightly contracted during the intervals of pain. All the measurements above referred to were made when the cervix uteri was smooth, and the os dilated from 1 to 2 fingerbreadths, and before the escape of the waters. The results of these measurements agree with Ahlfeld's. The length of the fœtus in oblique presentations, I have placed in Table VII.

TABLE VII.

Measurements of the Fœtus in Oblique and Transverse Presentations.

Week of Pregnancy.	Length of Fœtus (my measurements).	Number of Cases measured by myself.	Length of Fœtus according to Ahlfeld.	Week of Pregnancy.	Length of Fœtus (my measurements).	Number of Cases.	Length of Fœtus according to Ahlfeld.
	Ctm.		Ctm.		Ctm.		Ctm.
40	24·5	9	25·5	32	21·2	2	22·2
39	24·2	9	25·37	31	19	4	22·1
38	24	10	24·6	30	19	2	20·5
37	23·6	6	23·7	29	18	2	20·3
36	22·7	8	24·4	28	...	...	20·7
35	23·25	2	24·3	27	19	1	20·0
34	23	6	23·9	26	19	1	19·0
33	22·5	2	22·8	25	...	...	17·0
				24	19	1	...

In these cases I did not obtain, as Ahlfeld seems to have done, invariably high figures (*i.e.*, high when compared to the measurements made in natural presentations), but, on the contrary, observed very considerable differences in the length of the fœtus. Ahlfeld very justly remarks that in consequence of the excess of liquor amnii, the child is not so much bent as where the waters are in moderate quantity, and that these children are often ill developed. My own observations have brought me to the conclusion—

1stly, That the definition of the length of the fœtus before birth is possible in the majority of cases.

2dly That the size of the fœtus serves as a test for determining the period of pregnancy, combined, of course, with other objective symptoms.

3dly, That by the length of the child, an approximate opinion may be formed of the size of its head.

4thly, That, in measuring the length of the fœtus *in utero*, Pfannkuch's observation is correct, viz., that the length of the child at first keeps pace with its weight, but towards the latter part of pregnancy the progress in length is slower, so that up to the thirty-fourth week the length increases by nearly 1 centimetre weekly, whereas after the thirty-fourth week it barely reaches ½ a centimetre. In some cases this law is even more strikingly illustrated, but I shall abstain from citing figures, not to prove tedious.

Ahlfeld's belief, founded upon his measurements of the fœtus, is, that the position of the child *in utero* does not depend upon the form of the womb, but that, on the contrary, the shape of the uterus depends upon the position of the fœtus. This is by no means a maxim; indeed it is impossible to agree with Ahlfeld, or to understand how such a powerful muscular organ as the gravid uterus could, with the usual amount of liquor amnii within it, have no influence on the position of the fœtus within. How otherwise can we explain those breech presentations which occur in consecutive pregnancies commencing with the first? This brings us to a very important question, the consideration of which would prove out of place in this article. I shall therefore simply remark that three causes—the womb, the fœtus, and the waters—have a mutual influence on one another.

Ahlfeld was the first to apply the pelvimeter for measuring the height of the fundus uteri, with the view of obtaining more correct results than could be arrived at by the use of the tape measure—a measure which, owing, as has already been pointed out, to the varying convexity of the anterior wall of the womb, produced by the position of the fœtus, and the unequal amount of subcutaneous adipose tissue, supplies very contradictory data when applied to different gravid women at the same period of pregnancy. Ahlfeld in his measurements places the patient on a flat bed, for the purpose of determining correctly the position of the soft parts above the symphysis pubis in their relation to the latter,—puts his index finger over the horizontal ramus of the pubes, so that the radial edge of the finger should be on a level with the upper border of the ramus, while the point of the finger just touched the symphysis pubis. Along the middle of the finger-nail a line is drawn with Indian ink, opposite which one extremity of the pelvimeter is placed, so that its point is applied ½ a centimetre below the upper border of the symphysis pubis.

TABLE VIII.

Measurements of the Height of the Fundus Uteri, and of its Breadth.

Week of Pregnancy.	Height of Fundus Uteri among Primiparæ.	Number of Cases.	Height of Fundus Uteri among Multiparæ.	Number of Cases.	Average Height.	Height of Fundus Uteri according to Ahlfeld.	Maximum Breadth of Uterus among Primiparæ.	Number of Cases.	Maximum Breadth of Uterus among Multiparæ.	Number of Cases.	Average Breadth.
Week.	Ctm.		Ctm.		Ctm.	Ctm.	Ctm.		Ctm.		Ctm.
40	25·2	53	25·6	48	25·4	26·1	20·1	47	20·8	46	20·4
39	25	37	24·8	32	24·9	25·7	19·9	32	20·9	31	20·4
38	24·4	41	24·6	32	24·5	25·0	19·4	30	20·1	33	19·8
37	24·0	32	24·3	21	24·15	24·48	19·7	25	20·1	17	19·8
36	24·0	24	23·9	19	24·0	24·0	19·5	21	19·8	16	19·6
35	23·6	17	24·0	13	23·8	23·5	18·6	15	19·3	11	18·9
34	23·5	12	22·0	4	23·2	23·4	19·1	12	19·2	5	19·1
33	22·3	9	21·5	2	22·1	22·3	18·5	8	18	2	18·3
32	22·2	9	21·7	4	22·0	21·7	18·2	7	17·2	4	17·9
31	22·1	6	21·5	4	21·9	21·6	18·2	5	16	3	17·4
30	23·2	5	19	2	22·4	21·5	18·5	4	15	2	17·1
29	23	2	20	2	21·5	20·2	18	3	15	2	16·8
28	21·3	3	19·2	4	20·1	20·3	18	2	15·7	4	16·0
27	21·0	2	17·3	3	19·0	19·1	16·5	2	15	4	15·5
26	21·0	2	21·0	1	21·0	19·0	16·5	2	...	...	16·5
25	21·0	1	...	...	21·0	16·9	...	...	...	...	...
24	19·0	1	...	...	19·0	15·0	15·5	2	...	...	15·5
23	19·0	2	...	...	19·0	...	15·5	2	...	...	15·5
22	19·0	2	...	...	19·0	...	15	1	...	...	15
21	19·0	1	...	...	19·0	...	14	1	...	...	14
20	...	...	...	...	...	...	...	...	...	...	...
19	...	...	17	1	17	...	...	...	13	1	13
18	...	...	...	...	...	...	...	...	...	...	...
17	...	...	...	...	...	...	...	...	...	...	...
16	...	...	13	1	13	...	...	...	13	1	13

Should the highest part of the fundus uteri happen to be at the side of the linea alba, the point on a level with it in the mesial line is determined, and the measure taken by means of the pelvimeter from that point to the symphysis pubis. In my measurements, I followed Ahlfeld's method, with this difference only, that instead of marking any line on my finger-nail with China ink, I simply fix my right thumb horizontally over the horizontal rami pubis, while with the right index finger (I stand to the patient's right) I make one point of the pelvimeter hook the fixed thumb, and press against its lower border. The thumb thus serves as an immovable point. With the left hand, the other extremity of the instrument is placed over the base of the xyphoid cartilage. The space between these two points being measured, I next place that extremity of the pelvimeter which had first been upon the xyphoid cartilage over the fundus uteri, and note the distance between it and the right thumb. For further proof that my thumb has been a steady and fixed point during the latter measurement, I again place one extremity of the pelvimeter over the xyphoid cartilage, and if I find my first and third measurements agree, it is proof of the correctness of my second. For the purpose, however, of observing to what extent the womb is enlarged, I have found it necessary to measure its greatest breadth. This was accomplished by means of the pelvimeter, the two extremities of which were placed at the sides of the uterus, and its greatest breadth noted. In head presentations, the maximum breadth of the uterus is at its fundus; in breech presentations, a little below the fundus.

It will be observed that the results of my measurements closely resemble Ahlfeld's, with this difference only, that towards the end of pregnancy the height of the fundus uteri was somewhat less in my cases than in Ahlfeld's, which exactly corresponds with my measurements of the child's length, and which may also be accounted for by the fact that I excluded from my tables all cases of narrowed pelvis, as well as breech presentations, in both of which the fundus uteri is raised higher than usual. It is evident, from the figures brought forward in Table VIII., as well as from Richelot's[1] and Ahlfeld's observations, that the fundus uteri keeps ascending to the very end of pregnancy, and that there is no descent of the fundus uteri during the tenth month. This rule is applicable, not only in regard to the majority of cases, but in regard to every case individually, whether in primiparæ or in multiparæ, always provided the patient is placed, during examination, in a horizontal posture. In determining the height of the fundus uteri in the erect posture, however, the descent of the womb is clearly noticeable. I have made several measurements which gave the following figures:—During the 40th week of pregnancy, the average height of the fundus uteri, on the patient assuming the vertical posture, was 22·5 centimetres; during the 39th week, 23·3 cent.; during the 38th week, 23·4 cent.; during the 37th week, 23·3 cent.; during the 36th week, 22·5 cent.; during the 35th week, 22. As I do not attach much value to this question, my measurements were somewhat limited in number; but the general results arrived at show that on the patient assuming the erect posture, during the last month of pregnancy, the fundus uteri generally stands two fingerbreadths lower than in the preceding month of pregnancy. On comparing primiparæ with multiparæ, we perceive that the difference in the height of the fundus uteri is almost inappreciable when the patient is placed in a horizontal position. In regard to the breadth of the womb, however, the difference is palpable, viz., in multiparæ the womb is somewhat broader, commencing with the 34th week of pregnancy. This coincides with Gassner's[1] observations, who discovered that a larger quantity of liquor amnii existed in multiparæ than in primiparæ. No conclusions can be arrived at in regard to the earlier periods of pregnancy, owing to the paucity of observations hitherto made. In constructing the following tables, only those cases were taken into account where there was no uterine contraction, and where the bladder was emptied. When the uterus contracts, the fundus ascends from 0·5 to 1 centimetre during pregnancy, and from 0·5 to 4 centimetres during labour.

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[1] Richelot, Zur Diagnostik der Schwangerschaft. Königsberg, 1868. Inauguraldissertation.

TABLE IX.

The Height of the Fundus Uteri in Breech Presentations.

Period of Pregnancy.	Height of Fundus Uteri.	Number of Cases.	Maximum Breadth of Womb.
Week.	Centimetres.		Centimetres.
40	28	1	21·6
39	26·04	5	23·6
38	26·16	3	21
37	25·6	6	20·7
36	24·8	7	21
35	23·6	3	19·6
34	24·5	1	20·8
33	24·0	2	21·5
32	24	1	19
26	20	3	...

In comparing the above-cited figures to those representing the height of the fundus uteri in head presentations (Table VIII.), it will be perceived that the highest ascent of the base of the womb in breech cases is 3 ctm. at the 40th week, and from 0·5 ctm. to 2 ctm. for earlier periods of pregnancy, although only normally developed pelves were included in our measurements. Consequently, in determining the height of the fundus uteri, it is necessary to pay strict attention to the presenting part, while it will only be after a much more extended number of observations in breech cases that strictly correct results can be arrived at. Moreover, one cannot ignore the considerable expansion in breadth of the womb—an expansion observed in primiparæ as well as multiparæ. In regard to slightly narrowed pelves, we have too few observations; these, however, going to prove that the fundus uteri is very little higher in position than in head presentations with normal pelves. For instance, at the 40th week, the height is 25·2 ctm.; at the 39th, 25 ctm.; at the 38th, 24·6 ctm.; 37th, 24·5 ctm.; 36th, 24·1 ctm.; 35th, 24·5 ctm.; 34th, 24·5 ctm.; 33d, 24·1 ctm.; 32d, 28·8 ctm.; 31st, 23·7 ctm.; 30th, 23·5 ctm.

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[1] Gassner, Uber die Veränderungen des Koerpergewichts der Schwangeren, etc. Monatsschr. für Geb. und Fr.-Kr., xix. pp. 31, 32.

In the earlier periods of pregnancy, the measurements do not differ from those of normal pelves.

With regard to oblique and transverse presentations, my observations are too limited to tabulate.

If the size of the unborn fœtus be compared with the height of the fundus uteri, and with the measurement of the child after birth, it will be seen, as already pointed out by Ahlfeld, what a remarkably constant relation they bear to each other, if head presentations are alone taken into account.

TABLE X.

On the Height of the Fundus Uteri, as compared with the Length of the Fœtus, before and after Birth.

Period of Pregnancy.	Height of Fundus Uteri.	Length of Fœtus.	Length of Child after Birth.	Period of Pregnancy.	Height of Fundus Uteri.	Length of Fœtus.	Length of Child after Birth.
Week.	Ctm.	Ctm.	Ctm.	Week.	Ctm.	Ctm.	Ctm.
40	25·2	24·4	48·4	31	22·1	21·9	...
39	25	24·4	47·5	30	23·2	19·8	...
38	24·4	23·9	46·6	29	23	20·0	38·0
37	24·0	23·4	46·5	28	21·3	20·0	...
36	24·0	23·3	46·5	27	21·0	19·0	38·5
35	24·6	23·1	47·5	26	21·0	19·0	...
34	23·5	22·7	45·3	25	21·0	...	35
33	22·5	22·7	...	24	19·0	18·0	...
32	22·2	21·9	43·1				

In exceptional cases the height of the fundus uteri equals the length of the fœtus; in the great majority of instances, however, if we exclude all cases of an excess of liquor amnii, which, in proportion to its quantity, lifts up the fundus uteri, it will be found that the height of the base of the womb is equal to one half the length of the fœtus. In determining, with a tape measure, the distance between the fundus uteri and the symphysis pubis, the average figures point to the gradual increase in size of the womb (Richelot, p. 16, *op. cit.*); but for that the differences in individual cases are too great, when compared with the slight variations in the measurements taken with the pelvimeter. Thus, according to my own observations, in 281 cases, measurements were made with the tape measure, and also with the pelvimeter, and the variations obtained with the tape measure extended from 2 to 13·5 centimetres, *i.e.*, in the seventh and eighth months there were differences from 2 to 11 centimetres; during the 9th month, from 3 to 11 centimetres; while during the tenth month, from 8 to 13·5 centimetres; whereas with the pelvimeter the variations were only from 2 to 5 centimetres, and that in a decided minority of cases; consequently three times less than in measurements taken with tape. It is impossible to take the navel as a fixed point from which to measure the fundus uteri, because the length between it and the symphysis pubis varies greatly in different individuals. According to measurements which I made, the distance between the umbilicus and the symphysis pubis in pregnant women varies from 15 to 22 centimetres, and it has therefore appeared strange to me how so cautious a practitioner as Hildebrandt of Königsberg could have advised his pupil Richelot in 1868 to resort to such measurements, while Schroeder, even in the last edition of his Text-book of Midwifery, published in 1874, measures the position of the fundus uteri by determining how many fingerbreadths there are between it and the navel.[1] From measurements of the uterus made by myself, I have arrived at the following conclusions:—

(1.) The employment of the pelvimeter for measuring the height of the gravid uterus gives more trustworthy results than the use of the tape measure.

(2.) The upper border of the symphysis pubis is the most correct and unchangeable fixed point from which to measure the height of the fundus uteri, beginning with that period of pregnancy when the womb can be distinctly felt above the pubes.

(3.) The height of the fundus uteri above the pubes is a trustworthy objective symptom of various periods of pregnancy in normal and in reducible oblique presentations, when the womb contains only one fœtus. In these cases it is necessary to note the presenting part of the fœtus, and the size of the pelvis.

(4.) For plural pregnancies, a different scale of measurements should be adopted.

(5.) In non-reducible, oblique, and transverse presentations, the height of the womb cannot serve to indicate the period of pregnancy. In these cases the length of the fœtus, determined according to Ahlfeld's method, will serve to point out the corresponding height of the fundus uteri.

(6.) In slightly narrowed pelves, the distance between the fundus uteri and symphysis pubis is almost equal to the height in breech presentations. In cases of greater pelvic contraction, we have nothing to rely upon but the state of development of the fœtus, as determined by Ahlfeld's method.

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[1] Schroeder, Geburtshülfe, 1874, pp. 100, 101.

ARTICLE II.—*Notes and Observations on a Case of Poisoning by Aconite.* By THOMPSON LOWNE, M.D., L.R.C.S.E., etc.

THE subject of the following notes, an elderly lady of spare habit, had been suffering for three weeks from an attack of shingles. Believing the pain attendant upon such cases to be neuralgic, and but little influenced by external applications, I had prescribed a mixture of tincture and infusion of cinchona, each dose containing, in addition, thirty minims of the succus conii; and my patient had been taking it for some days, when, on 4th January, at 1 P.M., I was hastily called to her assistance. I found her sitting up in bed, with a countenance expressive of anxiety, very restless, rubbing her stomach, and complaining of nausea, with great præcordial uneasiness. Thinking, from her attendant's statement, these symptoms might in great part depend upon too hearty a dinner but recently taken, I carried out nature's indication, and, by irritating the fauces, produced free vomiting, when a considerable amount of undigested food was ejected, to the temporary relief of the patient. Upon examining the vomit, the smell of camphor was apparent; this led me to taste the mixture, of which I was then informed the first dose of a fresh supply had been taken about an hour before dinner. Almost immediately the presence of aconite was readily recognised; and judging from the rapidity and intensity with which the tongue and lips were affected, I came to the conclusion it was present in considerable quantity. A terrible significance was thus given to the symptoms present; free and copious vomiting was immediately reproduced, and kept up livelier; with the assistance of warm water, the contents of the stomach were completely thrown off; nausea, burning heat in the mouth and lips, pain in the gums, extreme restlessness, and a sense of prickling and numbness in both trunk and extremities, were quickly followed by inability to stand or move, an indescribable sensation of sinking, and failure of the circulation. These symptoms continued to increase in severity until about 6 P.M., when the poisonous effects of the drug appeared to have reached their maximum of intensity. The patient's condition at this time was most precarious; both upper and lower extremities were cold, and bedewed with a clammy sweat. She complained greatly of pain in the wrists, of general numbness, and a want of power, and of having lost the little finger of left hand; the pulse became irregular and intermittent; the irregularity showing itself both with regard to rhythm and strength; the cardiac sounds were weak and indistinct; ultimately, pulsation in the left radial wholly disappeared, and in the right was frequently barely perceptible. But the character of the pulse at this stage was so anomalous, as frequently to give rise to hopes of returning cardiac activity, which were speedily removed by subsequent examination; the pupils were slightly contracted, and the patient was almost, if not wholly, blind; the voice was not affected, neither were the mental faculties in the least disordered; there was a tendency to sleep, or slight stupor, from which she could be aroused with the greatest ease; lastly, a slight amount of subsultus tendinum showed itself at the wrists, and the breathing became occasionally somewhat irregular, with an occasional catch, followed by a sigh. At 8 P.M., the patient began to mend; this showed itself at first rather in an expressed conviction on her part that she was better than in any appreciable improvement in the objective symptoms; nausea ceased, but the stomach remained very irritable; sight returned, and the patient frequently remarked that the faces of her friends had no colour, but were livid and transparent; then the general temperature of the body improved; the little finger returned; and lastly, but not until past 9 P.M., the pulse also began to improve in strength and rhythm, and I felt I could with safety leave the patient.

The treatment, after freely evacuating the contents of the stomach, consisted in the administration of brandy by mouth, and when vomited—as was latterly the case—by injection, alternating with doses of ammonia. About 12 oz. of the former and ʒij. of the latter were taken. Sinapisms were applied to the cardiac region, and shifted from one place to another; bottles of hot water to the extremities, and ultimately strong black coffee, were given, apparently with benefit. I may add, that, with a view to avoid the effects of shock from mental agitation, I carefully avoided letting the patient know the true nature of her case; and I am inclined to think, had the terrible word "poison," been mentioned, the termination might have been other than that which happily took place. On making inquiry, I found that, instead of succus conii, the mixture had contained ℥ij. of liniment aconiti; so that a dose of aconite, equivalent to 240 drops of the tincture of the present Pharmacopœia, or 40 drops of Dr Hemming's strength, had been taken, associated with about 1½ grs. of camphor—40 drops of the tincture and ℥iii. of infusion of cinchona. It may be asked whether these adjuncts exerted any influence of antidotal or otherwise beneficial nature. As to the camphor, great difference of opinion appears to exist as to its physiological action, especially over the function of circulation; but even if it be a vascular stimulant in small doses, yet the smallness of the quantity taken could hardly have excited any appreciable influence. The cinchona, however, did, I think, happily tend to produce recovery—for tannin, in consequence of its forming insoluble compounds with alkaloids, has been recommended as an antidote; and although most of these insoluble tannates are said to be ultimately digested, yet all the poison that had entered into combination with the tannin of the cinchona would have been vomited, and thus prevented from entering the circulation; neither must we overlook the important part the full meal played in conducing to the safety of the patient.

It would have been interesting to have observed the relationship—if any existed—between the cardiac and respiratory movements, to have frequently noted, and with numerical accuracy, the condition and frequency of the pulse, the temperature of the body, and the state of the pupil, etc.; but it will be readily acknowledged that the state of the patient, the alarm and agitation of friends and attendants, the sense of responsibility that rests at such a time upon the medical attendant, present a combination of circumstances and conditions by no means favourable to systematic or minute observation. The above notes, therefore, necessarily lay no claim to minute detail; but they are faithful transcripts, as far as they go, of the leading features of the case.

The patient on the day following appeared to have suffered but little from the ordeal through which she had passed, but for four nights her sleep was disturbed by, as she expressed it, disreputable persons who assembled round her bed, and especially by an old lady, who, starting out from behind the curtains, with a huge umbrella, assumed a threatening attitude, and caused her to awaken with a scream. This was repeated four nights, and the uniformity of the dream is curious. There was also a mucous sanguinolent discharge from the bowel, attended with frequent tenesmus; but as we have no evidence that aconite ever exerts an irritant effect upon the intestinal canal, this was probably dependent upon a purely local condition of the rectum, produced by the brandy-and-water injections.

ARTICLE III.—*On the Risks run by Obstetricians.* By Dr CAIRNS.

(*Read before the Obstetrical Society of Edinburgh*, 18*th Nov.* 1874.)

It is not my intention to enter into detail on all the risks incurred by that class of medical practitioners above mentioned.

I might, *e.g.*, allude to the risk run by the obstetrician of carrying away on his person a swarm of pulices, pediculi, and cimices—parasites which, however interesting they may be to the student of natural history, are by no means pleasant companions to those who are infested with them—especially the latter, which through the medium of the obstetrician may infest his whole house, baffling almost every means for their extermination. These and other risks, however, I pass over; I confine myself exclusively to the risk run of contracting certain diseases, more especially syphilitic disease—a disease from which, considering the dreadful effects which it produces both on the party immediately affected and on his offspring, we may all well pray God evermore to preserve us. It is certainly a matter of deep congratulation that this disease is but rarely contracted by accoucheurs on their attendance in confinement cases, but it occasionally is, and perhaps more frequently than is generally believed, owing to reasons for concealment which I need not mention, but which must readily occur to every one present. The case which I purpose laying before you is as follows:—About nine months ago a medical man was confined to bed for about a month from what his medical attendants considered to be a case of blood-poisoning, but which he himself considered to be a case of scarlatina, caught from a little patient whom he had recently been attending for a very virulent attack of that malady. Be this as it may, the case presented all the usual symptoms of scarlet fever, followed after convalescence by desquamation of the cuticle and the formation of nearly a dozen successive abscesses in the neck. Two days after he was able to resume duty, he was asked by one of his pupils to go and examine a midwifery case in which he thought the presentation was abnormal owing to the long duration of the labour. He made only one examination, and found the presentation quite natural, and that the retarded labour was due to the fact that this was the patient's first labour and that she herself was above thirty-six years of age. The case terminated favourably. When my friend made his vaginal examination he had unfortunately a raw surface under the extremity of the nail of his right index finger; but before leaving, he, as usual, washed his hands thoroughly with soap and water, and afterwards allowed a stream of cold water to run over his hands from the sink waterpipe. Well, only a few days after this, he was seized with a most violent pain under the nail of the finger above mentioned, and two days after the occurrence of the pain there appeared extruding from under the nail a small, dark, soft excrescence, which increased so rapidly in size that in the course of four days more it had attained the form and dimensions of an ordinary-sized filbert. The pain was so excessive that it was deemed advisable to wrench off the nail and apply strong nitric acid, both of which were accordingly done. The excrescence in three days sloughed away, but by that time—*i.e.*, two weeks only after the vaginal examination had been made—there appeared a copper-coloured eruption which covered the entire body, it being most marked on the face, trunk, and upper extremities. In the course of a few days after the appearance of the eruption the eyes became so much affected as to occasion intense pain, and greatly impaired vision, loss of hearing, loss of smell, falling out of the hair, and most intense periostitic pains, all of which still continue in a greater or less degree. Sore throat also has existed ever since the contraction of the primary sore, and still continues. The primary sore is not yet healed. A portion of the distal phalanx has come away, but there still remains the largest portion to come, but, not being yet quite detached, it cannot of course be removed.

The student who attended throughout the whole case most fortunately escaped all injury. This may lead to the supposition that the patient after all was not suffering from the disease to which the foregoing remarks point; but this objection is easily met by the fact, that he had no abrasion of skin, and moreover by the two following facts, that the child died in a few days covered from head to foot with a syphilitic eruption, and that the parents candidly confessed that they were both suffering from venereal disease.

I now conclude with a few remarks, *1st*, As regards the character of the primary sore. In this case it was neither indurated at the base nor umbilicated, and yet followed by secondary symptoms, and these of a very violent character.

2d, The primary sore occurred at a much earlier period after contact with the poison than generally happens.

3d, The secondary symptoms showed themselves very much earlier than they usually do. Was this owing to the natural susceptibility of the patient to the poison, or to his being less able to resist its action owing to his illness immediately before the contraction of this disease? In my opinion, both causes may have operated, but I leave my superiors to determine.

The most important points for consideration, however, are, Should a woman, when about to be confined, be compelled, under the pain of a heavy penalty, to inform the accoucheur of her own accord that she is suffering from venereal disease? I think she ought.

For, consider the moral effects of the contraction of this disease upon the poor practitioner. If he should happen to be a virtuous man—and so far as my knowledge extends most medical men are—or should he happen to be cursed with a morbidly sensitive disposition, the mere fact of his having contracted such a disease is extremely apt either to render him a lunatic or a suicide, more especially if he is married, and still begetting children. But should such a direful result as this happily not follow, he is still doomed to suffer much, very much, cruel wrong. He loses his patients—generally the best ones; he goes to the country to be out of the way for a time; but on his transit by railway, the moment his face is seen every passenger in his compartment hurries off to another, anxious to be away from the presence of such a pestilential being; wishes lodgings when happily he reaches his destination, but no one will admit him with that smallpox-like face; is obliged to return home so downcast and dispirited, and so utterly undone in body, that he sighs for relief from this earth altogether.

Such was the case with the poor patient referred to.

Good patients lost—his character assailed and maligned—his feelings tortured and his presence shunned, no marvel that he ardently longed for death as he did from his bodily and mental sufferings—sufferings occasioned by a wretch who raised him in the dead hour of night to attend her, knowing well as she did that she had no fee to give to her medical attendant, except that of the loathsome disease under which she was labouring—a fee which would rather not have been received. I might go on enlarging on the baneful effects of this disease contracted by obstetricians, but I think I have said enough to entitle them to the deepest sympathy and praise for the gratuitous services which they render in many cases, considering the terrible risks which they run in so doing. And now, in conclusion, what means should be used by accoucheurs in order to avoid contracting this disease?

These means I shall simply state, without entering into details.

1st, Never attend a case of labour if there is any abrasion of skin on the right hand.

2d, If any abrasion exists on the right hand, place the patient in the appropriate position, and use the left hand.

3d, If not an ambidexter, have the right index finger in all cases covered with guttapercha tissue soldered together by chloroform, which tissue entirely protects the finger, and does not affect its sensitiveness.

4th, In all cases, after each examination, wash the right hand or left, as the case may be, with a solution of carbolic acid or any other good antiseptic.

5th, If, after all these appliances, a suspicious sore should occur, have the nail wrenched off immediately and caustic applied to the matrix.

ARTICLE IV.—*Bloodless Surgery.* By JOHN CHIENE, F.R.C.S.E., F.R.S.E., Assistant-Surgeon, Edinburgh Royal Infirmary; Lecturer on Surgery.

(*Read before the Medico-Chirurgical Society of Edinburgh, 6th Jan.* 1875.)

"BLOODLESS SURGERY" has been chosen as the title of this communication, because it is the expression generally adopted by surgeons when speaking of or writing about two proposals which Professor Esmarch of Kiel has lately, by his writings and practice, brought under the notice of the profession. These proposals are—

1st, The application, to the limbs more especially, of an elastic bandage before commencing the operation, which thoroughly empties the limb of blood.

2d, The application, as a compressing agent during the operation, of a piece of elastic tubing as a substitute for the tourniquet.

These proposals of Esmarch's must be considered separately, because, as far as I can see, they have nothing in common but *elasticity*.

First, The application to the limb of an elastic bandage, which thoroughly empties it of blood.

It has been said with truth, that the use of a roller-bandage, in order to empty the limb of blood, was long ago used by surgeons. Mr Spence, in a letter to the *Medical Times and Gazette*, in January 1874, pointed out that Sir Charles Bell, in 1821, speaks of the application to the limb before amputation of a roller-bandage; and Mr Erichsen, in the same journal, says that, twenty years ago, he had seen Mr Clover use the same means. To Professor Esmarch, undoubtedly, is due the credit of the *elastic* bandage, and to him is due the merit, if it is an improvement, of the proposal.

The action of this bandage is to empty the limb thoroughly. Of this there can be no doubt. In the words of its upholders, it renders the soft tissues " waxlike," and even " blanches the bones."

This thoroughness is in my opinion the chief objection to its use. The smaller arteries are so completely emptied, that the natural arrest of hæmorrhage, by the formation of an external clot during the operation, cannot take place, and therefore, after the compressing agent is removed, many vessels bleed which would have been temporarily closed by nature's external clot during the operation; and before the surgeon has time to close them by ligature, much blood is lost, more ligatures are required, and the operation is unnecessarily prolonged.

Further, the action of the bandage is so thorough, that there is not a drop of blood in the limb to guide the surgeon to the situation of the smaller arteries, and they cannot therefore be tied until the compressing agent above is removed. When this is done, every small artery bleeds freely; and for this reason, also, more blood is lost than would be the case if the limb had not been so thoroughly blanched. A slight oozing is necessary to guide the surgeon to those vessels.

By its thoroughness the elastic bandage is out of the question in cases of cancerous tumours necessitating amputation, and in cases of putrefactive suppuration, because by it deleterious materials may be driven upwards into the general circulation, and into the part of the limb through which the amputation is performed.

The plan of emptying the limb of blood by raising it vertically, which I first saw systematically practised by Mr Lister, and used by him for years before Professor Esmarch recommended the application of the elastic band, is preferable, because *it is not so complete.* An oozing from the smaller arteries takes place after the operation is finished, not sufficient to be in the least hurtful to the patient, but just sufficient to guide the surgeon to the bleeding points, and enable him to tie them before removing the compressing agent around the limb, thus saving blood. Further, this method leaves enough blood in the smaller arteries to allow an external clot to form, and nature's sealing process to take place; fewer ligatures are required, less blood is lost, and the bleeding points that require ligature after the compressing agent is removed from the limb, are diminished in number.

If the limb is raised vertically, it will be found that the pulse is diminished in force, but still perfectly distinct. I have found that gentle pressure on the main artery is sufficient to stop the pulse, at the same time in no way interfering with the venous return; and in addition to the raising of the limb and the rubbing of the limb towards the trunk, I have applied with advantage gentle pressure with the finger to the main artery before applying the tourniquet. With this precaution there is no hurry in screwing up the tourniquet, and with a single assistant to hold the limb vertically, the surgeon can at his leisure adjust the tourniquet.

In cases of necrosis, where no ligatures are required, the wound being stuffed, and a tight bandage applied before the compressing agent above is removed, the objections to the elastic bandage, due to the complete emptying of the limb of blood, do not hold good, and in them Esmarch's plan might be adopted. It is, however, here hurtful, because in cases of necrosis or caries, sinuses filled with putrefactive products are present. In such cases, the raising of the limb practically empties the limb, and is preferable on account of its simplicity.

Esmarch's elastic bandage is therefore inapplicable in many cases, and hurtful in some; and Lister's plan, in proper cases for the application of any method of rendering the limb bloodless, is to be preferred, because it is not so thorough. It is also simpler of application, and requires no special apparatus.

Second, Esmarch's proposal to use an elastic piece of tubing instead of the tourniquet of Petit.

I do not believe that it is more hurtful than the tourniquet, and the objections to its use, as stated by some surgeons, that it causes paralysis of the limb and sloughing of flaps in operations of long duration, are to my mind answered by Esmarch, when he says that he has used it in 300 operations without any bad results following. The chief objection to its use is this, that it cannot be relaxed and then tightened during the ligature of the vessels. In an hospital, with well-qualified assistants to apply manual compression if necessary, this is a matter of small importance; in private practice, without qualified assistance, it is a matter of paramount importance that the surgeon should be able, after the main arteries are tied, to relax the compressing agent, in order to allow blood to issue from the arteries to guide him to their situation, and then to tighten it again until the bleeding points are tied. For this reason alone, I believe the tourniquet to be preferable to the elastic band.

In œdematous limbs, in which the serum escapes from the wound, the tourniquet is apt to slacken. In these cases the elastic band accommodates itself to the decreased size of limb, and would be preferable, if it were not for the major objection already stated, that it cannot be relaxed and again tightened. It is much better in such cases to use the tourniquet, and, if necessary, tighten it during the operation; or an elastic pad can be used under the tourniquet, which, by its elasticity, will counterbalance the decrease in size of the limb, due to the escape of serum.

In using the tourniquet, to prevent nipping up of the skin, two large roller-bandages, or the large elastic pad, should be applied under the brass part of the tourniquet. It is not necessary to have any pad over the main artery under the strap of the tourniquet. The compression of the tissues, and necessarily of the bloodvessels by the strap, is quite sufficient to prevent hæmorrhage during the operation.

Another objection to the tourniquet is the difficulty which exists in fixing and releasing the buckle in common use. This is remedied by the use of a buckle, as used in braces and in fixing skate straps, which is easily applied, holds most firmly, and is most simply relaxed, enabling the surgeon to remove the tourniquet in a moment.[1]

These remarks refer specially to the value of Esmarch's proposals in major operations on the limbs—as in amputations and excisions. In minor operations—the removal of a toe or a finger, or a small tumour—it is of comparatively little importance what compressing agent is used during the operation, and in them it is certainly not necessary to use the elastic bandage to empty the part of blood. I am aware that the elastic ligature has even been used to encircle the abdomen and compress the aorta. It undoubtedly acts efficiently in preventing a flow of blood during the operation, but, in my opinion, the abdominal tourniquet is to be preferred, because it can be relaxed and again tightened during the ligature of the vessels. Esmarch has spoken of a modification of elastic compression in the removal of tumours from the trunk. The cases in which it can be used in these situations are so rare, that I need not detain the Society in discussing them. I further believe that these proposals of Esmarch will be adopted or discarded, according as they are useful in major operations on the limbs.

Twelve months have elapsed since Mr MacCormac of London, when advocating the value of the elastic bandage and elastic ligature, said that their acceptance had assumed an "epidemic" form. I trust that we have now seen the worst of this epidemic, and in the hope that these practices will not become endemic; I beg to suggest, for the consideration of the Society, that the elastic bandage and elastic ligature are not improvements in surgery, that Lister's method of emptying the limb of blood is preferable to Esmarch's elastic bandage, and Petit's tourniquet is preferable to Esmarch's elastic ligature.

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[1] This buckle has no spring, cannot get out of order, and simply acts by firmly compressing the strap between two slightly roughened surfaces. It is made by Mr Gardner, surgical instrument maker, South Bridge, Edinburgh.

ARTICLE V.—*On Prolapsus and Retroversion as Alternating Misplacements of the Gravid Uterus.* By FRANCIS IMLACH, M.B., Liverpool.

(*Communicated to the Obstetrical Society of Edinburgh by* Dr MATTHEWS DUNCAN, *9th December* 1874.)

PROLAPSUS uteri may occur any time within the first three or four months of pregnancy, and continue during the remainder of those first months, without danger to the pregnancy or to life; so also may retroversio uteri. But while it is exceptional for the former, it is not so for the latter, to prove a cause of danger.

These two states may with advantage be compared with each other: the former as a displacement downwards in the pelvic axis, the latter as a rotation backwards about a transverse axis. The two misplacements never coexist in the extreme state at the fourth month; but complete prolapsus is often associated with slight retroversion, and complete retroversion with some descent. Positions which result from combination of their various minor degrees are probably frequent, but of little practical importance; it is the successive occurrence of the two extreme misplacements, the substitution of one for the other in the same pregnancy, that is the chief subject of these remarks.

Both misplacements involve altered relations of the uterine attachments. Posteriorly, they involve like alterations. In prolapsus the rectum is not displaced, but Douglas's pouch descends with the uterus, and the subperitoneal fibro-cellular tissues, chiefly the utero-sacral ligaments, are drawn down to a like extent. In retroversion, the rectum also remains in its normal position, but the pouch follows the uterine cervix even to the symphysis pubis, and with it the underlying fibro-cellular tissues. And the extent of structural elongation must be nearly equal; for, suppose the uterine diameters to be 4 inches, and the retroverted uterus to have rotated through, say, 130°, then the elongation amounts to 4·5 inches, which corresponds with a very considerable degree of prolapse. Anteriorly, the vesical attachments are loosened—that is, extended—and the whole bladder displaced downwards in prolapsus, upwards and backwards so as to become extrapelvic in retroversion. Here, again, therefore, is similar structural elongation, differing only in direction. The lateral alterations are in contrast: in prolapsus there is simply downward extension of the broad ligament; in retroversion, there is twisting. In consequence of this torsion there must be shortening; hence, probably, previous extension favours subsequent torsion.

Both processes are usually reversible by natural means or artificial aid. Prolapsus consists in simple descent: ascent of the uterus is its natural course in pregnancy, and rectifies the misplacement; increased descent the natural course in abortion or labour at the third or fourth month. Retroversion consists in simple rotation about a transverse and nearly central axis; in rotation combined with more or less determinate deformation of the uterine mass, such as retroflexion and the irregular projections, described by W. Hunter; or in one of these, combined with some descent or prolapse: reverse rotation with ascent constitutes natural rectification during pregnancy; reversion with increased descent is the natural course of labour.

Each misplacement may recur after artificial rectification. As regards prolapsus, this is too well known to require any example. A case from Martin le Jeune[1] illustrates recurrent retroversion of the gravid uterus. A woman pregnant at about the third month, after a fall, suffered from retention of urine. The uterus was found retroverted, and was reduced without much difficulty; but on her getting up, the displacement was repeated, and again replaced. A recent case in my father's practice—which I did not see, but of which I had cognisance during the period of observation—is an example of alternation of the two misplacements in successive pregnancies. The lady has had three children born at intervals of sixteen and fourteen months. The first pregnancy was natural. At about the end of the third month of the second, the uterus became suddenly and completely retroverted, the os uteri being above the symphysis pubis. The retroversion was reduced without much difficulty; the patient remained in bed until the period of quickening, and after that the pregnancy was natural. At the same period of the third pregnancy, the uterus became extensively prolapsed; it was returned, and a Hodge's pessary inserted; she remained in bed for a few days, and the remainder of the pregnancy was natural. After the labours, which were all natural, there was no retroversion or other misplacement. The last case I have to mention illustrates the substitution of one misplacement for the other in the same pregnancy:—S. K., æt. 43, nine children and three miscarriages; says the womb came down during the early pregnancy of the eighth child. It returned at the period of quickening, and did not again descend for some months after delivery—not until she had begun washing and like hard work. The uterus again became completely prolapsed, somewhat suddenly, three weeks previous to examination. She had been standing washing, but not more than usual. Had not menstruated for two months. Uterus found to be the size of a large orange, and, with the bladder, extensively prolapsed; external os patulous, and its lips averted, eroded over a large surface, and covered by a lymphy pellicle, which is easily stripped off, and is seen to be continuous with the cervical plug. Nitrate of silver applied to the eroded surface, the prolapsed mass returned, and the patient kept in bed. Next day she complained greatly of dysuria and incontinence; the uterus was found completely retroverted, the os being above the symphysis pubis, and the fundus low down in the hollow of the sacrum. After various unsuccessful attempts at replacement, a large Barnes's bag was inserted into the rectum and greatly expanded. As this was also unsuccessful, it was left in all night, and after its removal the organ was easily replaced by manipulation—possibly being then in a more favourable pelvic diameter. The patient did well for five days, but then aborted of a fœtus over three months old. Six weeks after this, the uterus was found retroflexed, but the misplacement could be easily rectified by the finger.

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[1] Quoted by Bernutz and Goupil, Diseases of Women, vol. ii. p. 187.

As this is not a paper on retroversion or prolapsus, but on their alternation, I have refrained from referring to authorities for the statements of anatomical facts. The best anatomical description of retroversion of the pregnant uterus is that by William Hunter. The anatomy of prolapsus has been described, amongst others, by Dr Savage in his well-known work, "Surgical Anatomy of the Female Pelvic Organs."

ARTICLE VI.—*Ascites: Recovery after Paracentesis had been performed One hundred and thirty-three times.* By W. ALLAN JAMIESON, M.B., Berwick-on-Tweed.

INSTANCES of recovery from abdominal dropsy after one or several tappings are not rare. My friend Dr Cahill of Berwick has mentioned one to me, where a lady, then young, was tapped many years ago, and the fluid never reaccumulated. Dr James Duncan of Dundee has furnished me with notes of a case which occurred in his practice about five or six years ago. The patient had been tapped three or four times before she came under his care, and recovered after the third tapping; thereafter, the total duration of the ascites being two years. But I venture to think, that an example of recovery after such a number of tappings as 133, is unique: some notes, therefore, may prove interesting.

Mrs T., aged 66, is the wife of a miner at Heathery Tops, near Berwick. She was always temperate, and long healthy, and the mother of several children. About ten years ago she suffered from some affection of the liver—probably abscess; since, there is a history of pain and swelling in the hepatic region, which terminated suddenly, with the sensation of something having given way inside, followed by vomiting of purulent matter. She was then, and for a good many years subsequently, under the care of Dr Paxton of Norham. Her health remained good after this illness till 1869, when her abdomen began to swell, and at length, in April 1870, the fluid had increased so much, that she was tapped by Dr Paxton.

She came under my care on the 12th May 1870, and has been so ever since. Soon after I tapped for the second time. The fluid removed measured three gallons; it was pale straw coloured, alkaline, sp. gr. 1011, and was densely albuminous. On standing, it coagulated. Under the microscope, it exhibited a few epithelial cells. Throughout the case the character of the fluid continued the same; at times it contained a little blood, but this came from some omental vessel which had been pricked by the trocar. On feeling the liver after the removal of the fluid, it seemed nodular, though the area of dulness was not diminished. One spot, two inches to the right of the mesial line, corresponding to an elevation of the hepatic surface, was uniformly and markedly tender on pressure. There never was any jaundice. I thought once or twice after tapping, that I could feel something resembling a wrinkled ovarian cyst, and in 1871 I sent her into Edinburgh to see Dr Thomas Keith. He carefully examined her, and wrote me that his opinion was, that the case was one of simple ascites. All sorts of diuretics were tried, but without any beneficial effect. She was at times subject to attacks of diarrhœa, which did little towards restraining the accumulation of water, but weakened her so much as to necessitate her taking astringents. The urine varied in quantity; but, with the exception of an occasional deposit of lithates, was normal in quantity, and never contained albumen. There was no cardiac disease, and the lungs were healthy. The frequency with which paracentesis was required varied; at one time she was tapped as often as once a week, while sometimes she did not require it oftener than once in three weeks. It was always performed with the patient lying on her side in bed, the puncture being made with a large-sized trocar in the linea alba. The quantity of fluid was for a time pretty constant—about three gallons—but latterly was less. In 1873 her legs became very œdematous as the time for tapping grew near; and she suffered much from prolapsus of the vagina, interfering with micturition. She was several times tapped, during my absence from home, by Dr Maclagan and Dr Brown of Berwick, and Dr Aitken, now of Rome. She was tapped for the 132d time in June 1874, and did not again require the operation till the middle of September. Since then there has been no reaccumulation, her general health is excellent, she moves actively about her house, and expresses her delight when I call that she no longer requires me.

I am inclined to think the cause of the ascites was hepatic—some pressure on the portal vein, which has been gradually removed—for the external abdominal veins enlarged considerably during the time the dropsy was most urgent, while, as I have already mentioned, the liver felt nodular through the abdominal walls. The duration of the case, and other reasons, exclude cancer; nor was this irregularity of surface due to any true cirrhosis, but rather, I imagine, to atrophy of the hepatic substance, owing to obstruction of branches of the portal vein.[1] This obstruction may have been a remote consequence of the abscess from which she appears to have suffered, or was perhaps due to attacks of subsequent perihepatitis.

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[1] Murchison, Diseases of the Liver, p. 246.

ARTICLE VII.—*The Morisonian Lectures, delivered before the Royal College of Physicians of Edinburgh: Session* 1874. By J. BATTY TUKE, F.R.C.P.E., F.R.S.E.

LECTURE VI. (*delivered 3d April* 1874.)

IN the last lecture, it was said that the great majority of brain disturbances are due to changes in the blood-supply; but there exists, undoubtedly, a powerful minority in which we have no right to presuppose any such condition, in which we find no evidence of stasis. In certain of the pathological forms of idiocy, which have been so ably described by Dr Ireland of Larbert, various factors have been at work to establish defects, which in the individual are so far normal; that is to say, his brain is so constituted congenitally or by the accidents of childhood, that in him the cells and fibres are so mal-arranged, their constituent elements so imperfect, as to prevent the possibility of the exercise of psychical function to the extent of what is acknowledged to be requisite for a responsible member of society. But this condition in the individual is not conditioned by any peculiarity in his vascular arrangements—he is simply an imperfect human being; he is only relatively abnormal; his defective brain is but part and parcel of his otherwise enfeebled physique,—on a par with his half-withered limbs. The idiot is the extreme case, but there are idiots and idiots. What can the pathologist say about the idiot who manifests his idiocy simply by perversion of the moral sense,—the physically healthy man or woman who steals, secretes, lies, with no disturbance of his intellectual faculties, except that he cannot appreciate that he does wrong in lying, secreting, and stealing—he cannot, as it were, disapprove of his own proceedings? I am free to confess that no explanation of this section of insanity can be as yet afforded by morbid anatomy. Argument from analogy only warrants us in presupposing a deficiency or mal-arrangement of one or other of the brain elements. This condition must in the meantime be relegated to the somewhat long category of *opprobria medicinæ.* I am not desirous of begging the question; ignorance is simply admitted as to the physical cause of a morbid condition.

Taking a further step, can we give a pathological reason for those rare cases of simple melancholy without excitement which occasionally present themselves? or of those in which a simple condition of delusion is the only symptom? I would only ask you to remember how very seldom such cases are met with without some evidence of brain and general constitutional disturbance: for my own part, I am somewhat sceptical as to their separate existence.

But there is yet another section of brain lesions which may be produced without any causating modification of blood-supply. I allude to changes in the amount and nature of the neuroglia, changes which are due to morbid processes commencing primarily in its own constituents: these may be, in fact generally are, followed by vascular disturbance, and accordingly great difficulty exists in determining whether they are causes or effects. We have to depend mainly on the clinical history of the case in forming an opinion. I do not propose now to go into symptomatology in any detail, but will merely say generally, that when the earliest symptoms present a gradually increasing apathy, torpor, and stupidity, quickly followed by general bodily symptoms, loss of expression, heaviness and immobility of the facial muscles, a slobbering mouth, speech thick and slow, impairment of reflex action, considerable general anæsthesia, hæmatoma auris, and the passage of fæces and urine apparently involuntarily, we may look to the neuroglia as the seat of primary lesion. It is a condition of true primary dementia, which may perhaps best be described as in every way lacking acute symptoms, but accompanied by rapid decadence of the general system. It is closely allied to what is often spoken of vaguely as organic brain disease. In all such cases we find a sclerosis, and generally a colloid degeneration of the neuroglia.

But undoubtedly the neuroglia is subject to certain changes due to congestion and stasis, which must be regarded as the cause of chronic insanity. We have a terminative as well as a primary dementia; the former is infinitely more common, but may be referred to a condition or conditions somewhat similar to what occur in the primary form of the connective tissue, in addition to lesions of cells and fibres. But before considering the period of incidence, we will as shortly as possible discuss what are the morbid conditions of the neuroglia.

When we remember the differences of consistence shown by various brains immediately on their removal from the skull as tested merely by the sense of touch, we cannot fail to recognise, even by this imperfect test, that the connective tissue or packing material, as it may be called, must be the element which suffers in such conditions as œdema, hypertrophy, or atrophy. This is confirmed by microscopic examination, for by this means we are able to demonstrate many various forms of degeneration in the neuroglia. One of the processes to which these sections are submitted, that of staining with carmine, produces naked-eye evidence of a healthy or unhealthy condition of this substance; for if a section takes on a very deep colour, it may be taken for granted that there is an increase of the fibrillar element of neuroglia, and if the converse happens, that its protoplasm is changed in either quantity or quality.

The following are the different forms of disease to which the neuroglia is liable:—First, a general increase of its protoplasmic material; second, a disseminated sclerosed condition; third, a form of sclerosis which has been termed miliary sclerosis; fourth, an increase of its finely fibrillated texture; fifth, a colloid form of degeneration; and, sixth, atrophy. A most interesting instance of the first-named condition came under my notice about eighteen months ago, in a congenital idiot,[1] whose brain weighed sixty ounces: there was a manifest difference between the bulk of the two hemispheres, for, when carefully separated, the right half was found to weigh 30¼ ounces; the left, 23½ ounces; a difference of 6¾ ounces. It would be uncalled for now to enter on the full details of this case; suffice it to say, that the left side of the body was affected by atrophic hemiplegia. In the hypertrophied hemisphere (the right), the nerve fibres were seen lying in fasciculi, consisting of four, five, or six strands, separated from one another by very clear plasm, in which were more than normally numerous nuclei, and a firm fibrillar structure. This condition was best marked in the occipital lobe, less so in the parietal, and in a still minor degree in the frontal; in fact, it was coexistent with the degrees of hypertrophy of the several lobes. It may be regarded as a general or diffused glioma, and to its presence may be due the bulging brain of certain epileptics; and I would further suggest the probability that the weighty brain, not unfrequently met with in idiots who in infancy had been the subjects of hydrocephalus, may be mainly composed of this material. It must be admitted, however, that further proof is needed to establish this theory.

The second form of sclerosis is that which has been described by Rokitansky, Leyden, Charcot and Bouchard, Rindfleisch, and others, as gray degeneration, or more lately as *sclerose en plaques dissemenées*. It is manifest to the naked eye as light gray tracts of various shapes and sizes, which occasionally present themselves in sections of brains of chronic dements; it is more frequently observed in the motor tract, in the pons, medulla, and in the spinal cord, than in the white matter of the hemispheres; where, however, it does occasionally occur in widespread areas. When it is present great proliferation of the nuclei always exists, and we seldom fail to demonstrate its contiguity to a vessel or vessels around or upon which nuclei are likewise proliferated. These opaque tracts cannot be cleared up by any known agent; in them nerve fibres are to be seen partially or completely atrophied according to the stage of the disease; in transverse sections the axis cylinders are indiscernible, and the field of the microscope is occupied by a finely molecular and fibrillated material embedded in a cloudy homogeneous plasm. In this matrix the proliferated nuclei exist somewhat enlarged, and atrophied nerve fibres occasionally project raggedly into it. It appears to me that the views of Rindfleisch are most probably correct as to the genesis of the lesion: he holds that the nuclei on and around the vessels first become proliferated, that this is followed by increase of the nuclei of the neuroglia, and the development of a morbid plasm, which is in all probability modified connective tissue.

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[1] See Journal of Anatomy and Physiology, vol. vii. May, 1873.

The disease termed miliary sclerosis was first described by Professor Rutherford and myself in the Edinburgh Medical Journal for September 1868. Its presence has since been noted in various forms of nervous disease by Dr Kesteven, Herbert Major, and others; and I observe that it has lately been claimed as an undescribed pathological condition by Dr Hun, Pathologist of the New York State Asylum, which to me is gratifying, as confirmation of my observations by a perfectly unprejudiced and independent observer.

Miliary sclerosis is a disease of the nuclei of the neuroglia, and its progress is marked by three stages. In the first, a nucleus becomes enlarged and throws out a homogeneous plasm of a milky colour, and apparently of a highly viscid consistence, for the long axis of the spot is almost always in the direction of the fibres, which are displaced by its presence instead of being involved in it; thus indicating that its density is considerably greater than that of the cerebral matrix. In the centre of these semi-opaque spots a cell-like body is generally discernible, possessing a nucleus; this is the original dilated nucleus of neuroglia. In the largest patches more than one cell can be seen; whether these arise from division of the first nucleus involved, or from the original implication of more than one nucleus, has not been determined; but from the fact that multiple cells are seen only in the largest spots, it is most likely that the latter hypothesis is correct. Occasionally several neighbouring nuclei become diseased simultaneously, coalesce, and form a multilocular patch of considerable extent; the largest spot which has yet been figured is the one-fortieth of an inch in its longest diameter. During the second stage of development the morbid plasm becomes distinctly molecular in character and permeated by fibrils. It is probable that at this period a further displacement of the contiguous tissues takes place, as a degree of induration of the compressed fibres and bloodvessels which curve round the diseased tract is indicated by the increased amount of colouring material which they then absorb. At this stage the morbid tracts present the following appearances. As a rule the spots are unilocular, occasionally bilocular, and in rare instances multilocular; but whatever their condition in this respect is, they possess the same internal characteristics. A thin section prepared in chromic acid viewed by the naked eye shows a number of opaque spots irregularly distributed over the surface of the white matter; they are best seen in a tinted section, as they are not colourable by carmine. When magnified by a low power they have a somewhat luminous pearly lustre, and when magnified 250 and 800 diameters linear they are seen to consist of molecular material, with a stroma of exceedingly delicate colourless fibrils. They possess a well-defined outline, and the neighbouring nerve-fibres and bloodvessels are pushed aside and curve round them. In well-advanced cases the plasm seems denser at the circumference of the spots than at their centre, and a degree of absorption of the contiguous nerve-fibres is evident; this solution of continuity is only noticeable at the point where the lateral expansion is greatest.[1] The spots are generally colourless, but in some instances they are of a yellowish-green tint, which may be attributable to chromic acid. They vary much in size; multilocular patches are 1-50th of an inch to 1-100th of an inch in diameter, the unilocular ones from 1-200th to 1-800th of an inch. As many as eleven locules have been noticed in one patch, separated one from the other by fine trabeculæ of tissue.

The nervous tissue of a section containing spots of miliary sclerosis in the second stage, when removed from spirit and allowed to dry, shrinks from the diseased patches and leaves them elevated and distinctly separated from it, so much so that they can be picked out with the point of a knife.

It is still doubtful whether this lesion can be detected in recent specimens. In two cases, in which, by the chromic acid process, miliary sclerosis in the second stage was demonstrated, the recent white matter, when squeezed out under a covering-glass, exhibited spaces containing a clear material in which some rounded nucleated cells were visible. It is, however, only by prepared sections that its presence can be definitely ascertained.

In the third stage of miliary sclerosis the molecular matter becomes more opaque and contracts on itself, the boundaries become puckered and irregular in outline, and the material often falls out of the section, leaving ragged holes. These holes cannot be mistaken for empty perivascular canals, which, whatever their size, are smoothly rounded or oval. When in this condition the morbid products of miliary sclerosis are distinctly gritty, effervesce immediately on being subjected to the action of nitric acid, which produces no such appearances as are evolved by its application in the second stage.

The very fine fibrillar stroma of the neuroglia, which in health can with difficulty be distinguished by the aid of the most powerful lenses, often presents itself in thin sections prepared from the brains of the insane as a distinct meshwork under comparatively low powers. The spider-like cells of Deiters show themselves with great clearness. This condition is best marked in general paresis, and more especially at and near the line of demarcation between the white and gray matters. I see that Subimoff claims this as a new discovery, but it has been well known in this country for some years. This reticulated state of the neuroglia appears to have a great tendency to displace the cells and fibres, which become aggregated, with spaces intervening, composed of altered connective tissue. It must be regarded as a very important and interesting brain lesion.

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[1] See Bucknill and Tuke's Psychological Medicine, plate x. figs. 2 and 3.

Lastly, we come to a condition of the nuclei of the neuroglia to which I have been in the habit of applying the term colloid degeneration. I do not think it is a good term, but have sought vainly for a better. It is one of the most important degenerations of the neuroglia, not only on account of its degree, but of the frequency of its presence. It and miliary sclerosis are the lesions spoken of as ever present in true primary dementia.

In its earliest stages this abnormal condition shows itself in circumscribed semi-translucent spots scattered irregularly over the surface of the section, varying in size from the 1-4000th to the 1-2000th of an inch in diameter; they have well-defined irregular edges, and their contents are molecular in appearance. In fresh specimens, however, this molecular appearance is not observable, and colloid bodies appear as round or oval in form, having a distinct wall containing a clear homogeneous, transparent, colourless plasm, and occasionally showing a small nucleus, but no nucleolus. Colloid bodies are not colourable by carmine. They appear first in the white matter immediately contiguous to the cortical substance, but as the disease advances they become diffused outwards and inwards. In extreme cases, the appearance of sections containing them may best be compared to a slice of sago-pudding,[1] for they exist in such large numbers as almost completely to fill the field of the microscope, separated slightly from each other by a fine granular material. Although readily recognisable when set up in Canada balsam or turpentine, the characteristics of colloid degeneration are best brought out by glycerine. I feel strongly inclined to regard this as a form of degeneration of the nuclei of the neuroglia; it is first seen and is best marked in the white matter, but in certain specimens in which it occurs in the gray matter cells have been seen which are undergoing, or have undergone, changes in many respects resembling those noticed in the nuclei of neuroglia. It is not associated with proliferation of nuclei. Careful study of a large number of specimens leads to the conclusion that the nuclei are the original seats of the disease, for in all cases in which colloid degeneration shows itself they are to be seen more or less departing from normality; in fact, it may be safely stated that they are always unhealthy, and appear to merge gradually into the colloid condition.

There is one morbid product found in the brains of the chronic insane, and in the spinal cord of epileptics, of which I can offer no explanation as to its origin. These are the amyloid bodies of Virchow. He appears to believe that they are normal constituents; but when you compare the various preparations on the table, it is difficult to understand how the position can be maintained, more especially as they have never been found in any section of healthy nerve tissue which has come under my observation, or that of any histologist who has written on the subject. Like colloid, amyloid is not a good term. They are to be met with in the substance of

[1] See Bucknill and Tuke's Psychological Medicine, plate x. fig. 8.

the convolutions and cord, but their chief seat appears to be the extreme periphery, bound down by the pia mater.[1] I met lately with a very beautiful instance of this condition in the case of a man who, for forty years, had been the subject of chronic mania. The outermost layer of the gray matter was represented by a distinct stratum of these amyloid bodies, which were diffused very generally over the whole surface of the convolutions retained *in situ* by the pia mater. It can only be suggested that they are a modification of the granules of the granular layer.

I must refrain from speaking of many forms of lesion, which, although interesting, do not appear to have such direct bearing on psychical health as those just described. We will therefore, in a few sentences, consider the probable influence which the various lesions described have on the arrangements of the constituents of the hemispherical ganglia. Enough has been said at various times as to the primary effects produced by modifications of blood-supply in the induction of acute insanity, and I will now only refer to degeneration of the cells and of the neuroglia resulting from it, as influencing the chronicity of insanity. In the first place, when we consider that there is strong evidence before us that the vessels of the brain are for long periods dilated far beyond their normál calibre, and when we remember how very highly vascular an organ it is, it is self-evident that there must be loss of cerebral tissue concomitant with the increase of blood. This is to some extent provided against by the displacement of the cerebro-spinal fluid, but this fluid is far too small in amount to represent a bulk of matter equivalent to, say, a doubled amount of blood in the encephalon; and we are therefore compelled to the conclusion that one or other, or all of the solid structures, suffer in a greater or less degree. The unoccupied space represented in this diagram, as affecting an individual vessel, is the result of a loss of brain substance; and if such a condition extends to all the arteries of the superior convolutions, the total amount of tissue absorbed must be very considerable. We find dilated perivascular canals in both white and gray matter, but, as a rule, much more frequently in the former than the latter—in consequence of the larger size of the vessels which permeate it. That the effect of such a condition of the cortical substance must be very prejudicial to psychical health is certain, inasmuch as its cells, nuclei, and fibres are displaced, and the continuity destroyed. In the white matter the normal relations of the fibres is materially interfered with, and absorption of neuroglia produced. The evidence of this is to be found in sections of a convolution of a confirmed case of congestive insanity, for in such we never find the regular fan-like arrangement of cells and fibres which is the natural condition of health; there are clear interspaces, the cells and nuclei are irregularly distributed, and the fibres are pushed together into fasciculi.

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[1] See Bucknill and Tuke's Psychological Medicine, plate ix. fig. 6.

Superadded we have the pent-up exudates and products of waste acting in the manner spoken of in the last lecture.

In regard to the microscopic tumours, miliary sclerosis, and colloid bodies, the same remarks apply—the continuity of fibre is destroyed, and foreign bodies are present, implicating the normal anatomical condition.

Gentlemen, were I to enter on the full details of this section of my subject, I should far exceed my limits. The demonstration of the various lesions must suggest to you their probable results. When I commenced to write these lectures, I thought we could easily overcome the subject, but it has widened and widened, and I am compelled to stay my hand at this point. It must be confessed that I have failed in many respects, but I ask you to remember that I have been cramped by the necessity of concentrating my remarks: certain subjects have been slurred over which should have been thoroughly discussed; others have been enlarged on, in consequence of personal interest in them, which might have been deferred. We have but fluttered around the outskirts of this magnificent field of inquiry. But, if I have given you any stronger grounds of belief in the somatic nature of what we call insanity than you previously held, if I have pushed my specialty one degree further into the realm of positive medicine, the main object of these lectures has been obtained.

And now, Mr President and Gentlemen, in bringing my lectures to a close for this year, I must, in the first place, beg your pardon for making promises which the time at my disposal has not enabled me to fulfil. I proposed, in the first lecture, to correlate Ferrier's experiments with the results obtained from pathological examination of the brain of the insane; in my last lecture I promised to take up such an important pathological question as the action of the increase or decrease of the cerebro-spinal fluid on pathological conditions; but the *edax rerum* forbids anything but a superficial review of these questions; and as they are questions of too great dignity to be so reviewed, we must relegate them to the future—and possibly the delay of a year will not militate against the value of the inquiry. We have yet to obtain the full details of Ferrier's further experiments; and the subject of the influence of the cerebro-spinal fluid under morbid conditions could not be undertaken without careful consideration of Leyden's monograph on the subject.

I only ask you now to review with me briefly the general outcome of my lectures. We started with the proposition that insanity consists in morbid conditions of the brain, the results of defective formation or altered nutrition of its substance, induced by local or general morbid processes, and characterized especially by non-development, obliteration, impairment, or perversion of one or more of its psychical functions. Holding this as a proposition to be proved, I sought by every means at my disposal to demonstrate, as a necessary first step, that the arrangement of the constituents of the brain was definite and distinct; that their normal relationships were fairly established; and that their individual histology was as far within our ken as that of the majority of other tissues of the body. As a corollary, it was stated that, in the brain, as in the other organs of the body, the normal exercise of function is dependent on a perfect maintenance of the anatomical relations of the component structures. We next proceeded to consider what were the somatic conditions which involved the brain primarily or secondarily, tending towards a solution of the continuity of its constituent elements. In the last two lectures we have devoted our attention to the abnormalities of structure which are constantly found in the brains of the insane. In this procedure, I claim to have followed the course of the teacher of medicine in regard to other organs of the body; and I further claim to have never, in one single instance, failed to support statements by absolute demonstration, whether in regard to the appearances in health or in disease. This principle has been so strictly adhered to, that many points which appeared almost warrantable to advance as definite statements, have been withheld, because they were only *almost* warrantable.

Have we then asserted for insanity a position to which is due the term, *pathological entity?* In what have we failed? Is it that we have not succeeded in bridging the chasm between pathology. and metaphysics? This we have certainly not effected, nor is it demanded of us as physicians. *Ne sutor ultra crepidam* is a good old saying, and is strongly pertinent to our position; for, were the physician to hold his hand till he had thoroughly correlated metaphysics with physics, he could never feel justified in administering tonics to the dyspeptic, or stimulants to the depressed patient. To quote the nervous words of Griesinger, "It has been supposed that the study of mental disease was distinguished by some difficulty *sui generis*, and that the study of ordinary medicine had no direct bearing upon it—that the only entrance to psychiatrie lay through the dark portals of metaphysics. And yet the other cerebral and nervous disorders which, with the so-called mental diseases, form one inseparable whole, have not, so far as I am aware, been hitherto in any degree elucidated by metaphysics." It is in the mortuary and the work-room that the arcana of cerebral pathology will be disclosed; the section-knife and the microscope will, at no very distant period, lay open secrets which the iteration of theory by the abstract philosopher can never discover.

Feeling certain that there are points connected with the normal anatomy of the brain which demand elucidation, and which have important bearings on its pathology, I propose to conduct a series of experimental inquiries during the next few months, which I hope may form the basis of next year's Morisonian lectures. Our knowledge of the angeiology of the encephalon is still imperfect in respect of the differentiation between arteries and veins in the cerebral convolutions. The question of an encephalic lymphatic system is an open field of inquiry, and I even experience doubts as to the correctness of the generally recognised views of the relations of the cerebral investments. To the study of these subjects my attention will be directed before I next address you; and when I add that the sympathies of Dr M'Kendrick and Mr James Dewar have been enlisted in a work which demands the co-operation of the physiologist and the physicist, it may be fairly hoped that by next session something more worthy of your attention will have been evolved than what my unaided efforts have as yet produced.

ARTICLE VIII.—*Case of Poisoning by Strychnia, treated by Chloral Hydrate, and followed by Recovery.* By J. C. OGILVIE WILL, M.D., Assistant-Surgeon to the Aberdeen Royal Infirmary.

ON 26th May 1873, between half-past two and three o'clock, I was hurriedly summoned to G. S., a druggist's apprentice, æt. 18. Two medical men were already in attendance, and one of them, Dr Murray, informed me that the lad had, between 1.30 and 2 o'clock, after partaking of dinner, swallowed a quantity of strychnine, the exact amount I am unfortunately unable to state, the poison having been taken by chance from a bottle. According to the patient's account, it was five or six grains, "at any rate not less than four." The crystals had before leaving his shop been reduced to powder, and a packet containing strychnine in this state was afterwards found in one of his pockets. A few minutes after he had taken the poison he began to feel tingling in his hands and arms, followed by rigidity, and spasms soon set in, when his landlady sent off for Dr Murray, who at once administered an emetic and tickled his fauces, and at length succeeded in producing a slight degree of vomiting, or rather retching; for in some notes of the case, for which I am indebted to Mr Laws, student of medicine, he states that "a slimy, watery fluid" was brought up, and, as the lad's dinner had been chiefly composed of milk, had actual vomiting taken place, the appearances presented by the vomited matter would have been very different.

On looking at the patient when I first entered the room, beyond flushing of the face, large drops of perspiration on his face, and matting of the hair arising from the profuse perspiration, nothing unusual was observable; but a minute or two afterwards, on my touching him, symptoms indicating strychnine poisoning presented themselves. The appearances then observed were as follows: Great agitation and jerking, immediately followed by a loud, sharp scream, increased suffusion and redness of face, mouth open and stretched laterally, lips livid, eyes staring and much suffused, pupils dilated, head thrown back, spine and neck bowed, arms and legs rigid and cold. The fingers were straight, but bent at their metacarpal articulations, so that their points almost touched the palms of his hands; and the toes were also straight, but bent at their metatarsal articulations towards the sole, which was very much incurved. The muscular contractions were specially noticeable in the stump of the left leg, which had been amputated some years before, almost conveying the impression that the cicatrix would burst open. The attempts to respire were attended with great distress, and suffocation seemed imminent. The symptoms gradually disappeared, and the patient returned to the state in which I first found him.

The length of time between the fits varied, but during the time I watched him the intervals could seldom have exceeded five minutes. Mr Laws, a medical student, who took careful observations and notes of the case, states that "the spasms occurred in groups of two or three, following each other at intervals of about two minutes," then there was a longer interval, then another group, and so on.

During the intervals the patient was perfectly sensible—in fact, his senses were preternaturally acute, and the slightest noise in the room, the play of children on a green some yards from the house, and the falling of a plank in an adjoining workshop, were all noted as exciting causes in the production of spasms. After the cessation of the convulsion which I have described, I suggested the hypodermic injection of chloral hydrate; and on consultation with Dr A. Ogston, who had by this time arrived, and the others present, it was at once agreed to, Dr Ogston volunteering to fetch a syringe and Dr Murray to procure the chloral. During their absence a convulsion of such protracted severity occurred that both Dr Alexander Reith and I feared that life would be extinct before their return (at this stage the pulse was imperceptible at the wrist, and with my hand applied over the heart I was for some seconds unable to detect its action), but fortunately it also subsided, and the chloral being then at hand, but not the syringe, I at once administered 30 grains by the mouth, preferring it to emptying the stomach, as it seemed evident that poison sufficient to cause death had already been absorbed. Moreover, the convulsions were increasing in severity, and I felt thoroughly convinced that were the strychnine not rendered inert by an antagonistic agent, not many minutes would elapse ere death would ensue; and should the chloral not produce any amelioriation, the stomach might still be cleared of its contents when we obtained the means necessary for applying the chloral hypodermically. Fortunately, in a very few minutes the good effects of the chloral manifested themselves, the intervals between the spasms becoming elongated, and the spasms themselves lessened in severity; and the hypodermic syringe being by this time forthcoming, an injection of a solution containing 30 grains of chloral was given, and almost immediately afterwards a decided-change for the better was observed. The question of emptying the stomach was then considered; but as the chloral seemed to be exercising so beneficial an influence, we agreed to place our trust in the antagonistic properties it was exhibiting. Shortly after this I left, Dr Murray and Mr Laws taking charge of the case; but before doing so, I requested that should any vomiting take place, the matter might be kept. Unfortunately my suggestion was overlooked. Soon after the fits ceased for twenty minutes, but they again began to increase in frequency, and at 5 o'clock the same quantity of chloral was given by the mouth, and at 6.15 repeated, but the latter dose was followed by vomiting; at 6.30, 15 grains hypodermically, and in a few minutes the patient seemed to be thoroughly under its influence, becoming quite sleepy, and lying perfectly quiet, except when roused by noise or a spasm. From this time the fits gradually subsided, the last being seen between 11 and 12 o'clock, and he passed a comparatively quiet night. When I visited him late in the evening he was drowsy and quiet, but had occasional startings.

On the following morning he complained of weakness, exhaustion, and considerable pain in his back, and the parts where the chloral had been injected were tender to the touch; but in twenty-four hours this entirely disappeared, and no symptoms of any moment were discoverable. Next day he had so far recovered that he left for the country. The lad stated that he first felt the effects of the poison in his fingers, and then in his hands and arms; that during the spasms he suffered excruciating pain, with a feeling "as if his heart was coming into his throat," and accompanied by a sensation which he compared to "plunging through a wave when bathing."

The result of the treatment adopted in this case seems to me to be a proof that the antagonistic action of chloral hydrate to strychnia, proved, by the experiments of the Committee appointed by the British Medical Association to investigate the antagonism of medicines, to exist in the lower animals, also shows itself in man; and I think that it is strong testimony in favour of some of the conclusions arrived at by Dr Bennett and his co-workers, as given in the *British Medical Journal* of 23d January 1875. One it at any rate corroborates—"That after a dose of strychnia has produced severe tetanic convulsions, these convulsions may be reduced both in force and frequency by the use of chloral hydrate, and consequently much suffering saved;" and considering the undoubted severity of the symptoms, without laying much stress on the repeated statement of the patient that he had taken "not less than four grains of the poison," I cannot but think myself justified in asserting that it goes far to prove the correctness of another, viz., "That after a fatal dose of strychnia, life may be saved by bringing the animal under the influence of chloral hydrate."

ARTICLE IX.—*Notes on the Grouse Disease.* By ANDREW WILSON, Esq., Lecturer on Zoology and Comparative Anatomy, etc., Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 3d March.*)

IN the history of animal epidemics, probably no one series of phenomena has so powerfully attracted the common notice, and aroused the undivided attention of the pathologist and naturalist, as that included under the name of the "grouse disease." Indeed, the term has now come to be regarded as expressive of a mysterious lesion, or of a combination of ill-defined troubles, affecting this bird-genus in much the same way as instances of "plague" affect the human race. In either case the mortality appals us; and our inability to arrest or stay the spread and progress of the epidemic, together with our avowed ignorance of its causes or cure, only add to the unsatisfactory feelings with which we contemplate the entire subject.

The true and reliable mode of investigating such a subject, naturally appears to lie in the direction of pathological inquiry. First to ascertain the nature of the lesion; thence if possible to determine its cause; and, conjointly with the latter point or separately, to suggest from our pathology the means for prevention and cure. Such are obvious landmarks which plainly indicate the track of the investigator.

The present question, however, possesses certain peculiarities in consideration, which remove it in some degree from the natural province and sphere of the purely human pathologist. The investigator of the phenomena of disease amongst the lower animals must needs possess some knowledge of comparative anatomy, and of the normal appearance of parts and tissues in lower forms. What to the human pathologist might appear an abnormal phase of structure or function—as, for example, a high degree of vascularity of the tissues—may be, and in many cases is, a perfectly normal state of matters in certain tissues of the bird and of other lower vertebrates. And in other and allied points, the primary difficulty or care of the investigator consists in the task of determining what is really healthy, and what is abnormal, in the case before him. Experience in such duties can alone accustom the sight, assisted or not, to distinguish and recognise these essential conditions; and in some cases, a disregard of this important point has, I am assured, led to much misconception in otherwise excellent accounts of these and similar phenomena in the lower animals.

The object of the present paper is merely that of indicating the probable nature of the primary, or at all events of a very marked lesion observed in grouse which have perished from the so-called "disease." Opportunities for the examination of birds dying from the disease have been afforded me of late in comparative plenty; and although I cannot in any sense pronounce with certainty upon the exact cause or nature of the fatal lesion, the results of these investigations may be briefly detailed, if for no other reason than that they may in some measure aid the work of the future.

The subjects submitted to examination were found under such circumstances, and in such localities, as reasonably entitle one to believe that they had succumbed to the mysterious epidemic. They were found dead generally in numbers; and the appearances observed on dissection were for the most part of so uniform a nature, that this association in misfortune might be held to be due to a common cause. This cause, without undue straining, we might reasonably assume to be the presence of disease, and that, in all probability, of a specific type.

In most of the birds examined, a markedly congested appearance of the mucous surfaces of the digestive and respiratory tracts was observed. Repeated dissections and careful observation satisfied me beyond doubt, of the almost invariable presence of this lesion in birds which were forwarded as having been found dead under the circumstances already detailed. In three cases, the specimens of which were obtained through Professor P. A. Simpson of Glasgow, these appearances were especially marked, and were present in a higher degree than I had before seen them. But throughout previous observations, I had noted the presence of this condition; and I was led to the conclusion that, whatever its true nature or bearing upon the actual "disease" or primary lesion, this congestion was to be viewed as a pathognomonic symptom, and as a leading feature in such cases.

This belief was strengthened by the fact, that in those cases in which the congested appearances were absent or but faintly marked, death could more readily be accounted for by other causes, such as inanition, or the presence of parasites; these latter causes or features being generally in abeyance where the congestion was well developed.

These appearances were more or less defined in different birds; presumably according to the stage at which this particular lesion, *per se*, or in conjunction with other lesions, proved fatal. In the most typical cases, the appearances were similar to those observed in the human subject as the result of acute inflammatory action. Engorgement of the tissues, involving the deeper parts of these structures, was thus observed; the appearances being most notably developed in the lungs and respiratory surfaces generally; whilst the digestive system, chiefly towards its upper portion, exhibited similar evidence of inflammatory action. The lower portion of the digestive tract, from the gizzard downwards, did not appear, in the great majority of instances, to participate to any decided extent in these symptoms.

That these appearances were not due to putrefactive or post-mortem changes, I was careful to determine. The short time elapsing between death and examination in many cases in which these appearances were well marked, forbids such a supposition; and none of the attendant phenomena of decomposition could imitate the lesion alluded to, or could mislead the observer who closely scrutinized the appearances presented, and who was familiar with the signs of putrefactive change. The analogy of these appearances with the signs of recent acute inflammation in man, was equally obvious—so much so indeed, that one is at a loss to conceive any other series of phenomena which could at all correspond to the appearances observed in the grouse.

That this particular lesion was intimately associated with the cause of death, is a conclusion supported by the fact already mentioned, that in nearly every well-marked case of this congestion, no other lesion sufficient to account for death could be detected. And if one may be led to accord to this congested state a greater degree of importance than may hereafter be shown to be necessary or correct, an excuse may be found in the fact of its solitary, and apparently paramount, importance, as revealed on dissection.

Within the limits of a mere note, such as the present paper is meant to be, and as a purely preliminary and passing notice on this important subject, it would be almost unwarrantable to say much, or indeed anything, respecting the bearing of these observations on the great question at issue. But a few remarks, on the present aspects of the "grouse disease" controversy, may be permissible by way of conclusion, and especially with reference to the two chief theories which that controversy involves.

The first of these hypotheses is generally associated with the name of Dr Farquharson of London, who maintains that the lesion or lesions partake of the nature of a *contagious epidemic*, propagated by association or contact, and which, like epidemics in general, are presumed to have their origin in conditions of a more or less determinable kind.

The second theory, which has Dr Cobbold for its chief exponent, may be termed a hypothesis of *parasitism*. Dr Cobbold's researches on entozoa and helminthology, justly entitle his opinions on those matters to be received with respect; and he accordingly maintains that the grouse disease has its essential feature in the growth and propagation within these birds of parasites or entozoa; the result being to produce the "pining" condition, with the sequelæ of inanition, disease, and death. The chief parasite, or that to which the diseased condition is thus presumed to be mainly due, is a round worm of small size—the *Strongylus pergracilis* of Cobbold—which appears to select the cæcal appendages of the intestine as its abode. In the cæca, these worms are met with sometimes in very large quantities. The grouse tape-worm, *Tænia calva*, is also commonly present in the intestines; but I do not apprehend that the parasitic theory attaches to the tape-worm, or itself, any grave or responsible share in the production of diseased conditions.

Of these two theories I must unhesitatingly prefer the former; and the reasons for enlistment under the banner of the epidemic hypothesis rather than under that of parasitism, are chiefly those derived from the consideration, that the former theory fully comprehends *all* the conditions which the phenomena of the grouse disease may present; and further, that the theory may be applied to the explanation of points and facts, of which the theory of helminthiasis can take no heed.

We thus find that these birds are almost invariably affected by both kinds of entozoa; most specimens to a moderate extent, some to a very great degree. But it is exceedingly rare to meet with a bird in which not a single parasite of either kind is present. Out of many cases examined not a single bird was found actually free from the characteristic tape-worm, and in one or two doubtful instances only could it be asserted or suggested that none of the round worms were present.

That the presence of these parasites was unconnected with the death of the great majority of the birds examined, is, I think, proved by the want of any causal or obvious relationship between the infestation and the fatal issue; and also by the absence, in most cases, of the signs of fatal parasitism—such as inanition, producing the "pining" condition, actual perforation, morbid appearance of the muscular tissues, etc. No one can deny that the presence of parasites in large numbers undoubtedly causes death in a specific and readily understood manner; but this fact will not explain the nature of the cause or disease which operates so fatally, *without any exceptional development of parasites*, and certainly without the slightest appearance of inanition or other concomitant symptoms of helminthiasis. Outside the parasitic hypothesis, applicable as that theory is to a certain class of cases, there lies, I am convinced, the great bulk of fatal instances, the exact cause of which fatality must be sought for in some lesion analogous to that involved in the idea of the epidemic theory. We must thus account for the death in numbers of grouse which, with what one may consider a normal and natural degree of parasitism, yet die and succumb to disease, and which on examination present well-nourished bodies, firm, healthy muscular tissue, and other signs of presumed health.

Conversely, the lesions found after death, as exemplified by the foregoing observations, are perfectly compatible with the theory and suggestion of a contagious epidemic disease, which runs through a defined course, and which, could the animals be observed during life with the requisite degree of care and attention, would probably be found to exhibit a symptomatology analogous to that of an acute febrile attack. And this view may receive some support from its serving to explain the varying degrees of intensity in th appearances observed on dissection, by presuming that the atta may terminate fatally at various stages in its course; or that the less marked appearances seen in certain cases may be consistent with symptoms of resolution and amendment.

The oft-repeated statement that hunger and starvation have much to do with the cause and origin of the grouse disease, contains, I am persuaded, very little truth. The absence of symptoms of inanition in cases of unexplained death in grouse apart from parasitism, is the rule and not the exception; and certainly the phenomena of inanition have nothing in common with the more special symptomatology of the so-called "disease."

The etiology of the lesion forms one of the undiscovered mysteries of the question. Cold, damp, prolonged wet seasons; hunger, alone or in combination with the previous alleged causes, or with parasitism—have each and all been suggested as the causes of this disease. But this department of the subject may well be laid aside until the special pathology of the lesion has been first determined—this latter investigation may indeed of itself lead us to a true idea of causation.

It may not be straining analogical reasoning in an undue manner, if one may lastly suggest some possible relationship of that kind, between the well-known "cattle disease"—the pleuro-pneumonia of the veterinarian—and lesion of the grouse. The former affection has its seat in the respiratory tract; it is eminently contagious; it partakes of the nature of an acute inflammatory affection; and it is pre-eminently fatal in its character. And so with the grouse disease, which, though certainly not of so important or so grave a nature as the cattle epidemic, yet causes much regret and anxiety to the sportsman as well as to the naturalist.

Future observations, which I hope to have the opportunity of making in co-operation with Professor P. A. Simpson of Glasgow, together with the labours of other workers, may prove the tenability and correctness, or the error, of these briefly expressed opinions. I may state, that Professor Simpson, independently of these observations, and from his examination of many cases of diseased birds, has been led to adopt views in regard to this question almost similar to those which I have had the privilege of being thus permitted to express. The important question of locality in its relations to the "grouse disease," forms only one of the many aspects from which the question will have to be regarded by workers in the future. And if I may be permitted, I should like to re-echo the repeatedly expressed wish, that means and opportunity should be afforded to naturalists, by the combined influence of those who have the highest interest in the preservation of our moors and game, for the full investigation of this important subject—and such an expression I deem only a fitting conclusion to the views which, without dogmatism, I have been led to form and entertain.

Part Second.

REVIEWS.

Contributions to the Mechanism of Natural and Morbid Parturition, including that of Placenta Prævia; with an Appendix. By J. MATTHEWS DUNCAN, President of the Obstetrical Society. Edinburgh: Adam and Charles Black.

THE book before us is a collection of papers, all bearing more or less directly upon the mechanism of parturition, natural or morbid.

The term "mechanism," however, must, in reference to this work, be understood in its wide meaning as embracing our knowledge of the nature of the processes and the mode of action of the various forces which determine the onset of labour, and effect parturition; including, but not merely consisting of, a description of the manner in which the fœtus and placenta traverse the genital passage. All the papers are exceedingly valuable and highly interesting. Some of them are of surpassing merit, and far above praise.

The subjects treated are so varied and vast, and the amount of learning compressed into each paper is so great, that on every one of them an essay might easily be written. Our space, however, forbids us going into detail, and compels us to content ourselves with briefly alluding to a few of the more prominent papers.

As almost all the papers have been already published in this Journal at the time of their original appearance, our readers are acquainted more or less intimately with most of them. Besides this, several of them, having already been published in the author's work entitled "Researches in Obstetrics," were noticed by us at the time of the publication of that work, and consequently do not require to be again referred to here.

We observe, however, that all the chapters which were published in that work bear internal evidence of having been subjected to a rigid revisal, and, whenever the advancement of obstetrical science required it, of having been corrected and improved up to the present date.

It is much to be regretted, we think, that Dr Duncan had not attempted to work the individual essays more into a conglomerate whole, and have thus made the work bear more directly upon the mechanism of parturition. The value of these separate contributions, undoubtedly great though they be, would have been much enhanced had there been a better arrangement among, and stricter concatenation between, the various chapters which constitute the work. A good deal of repetition would have been thereby avoided, and a more clear and impressive idea of the subject would have been communicated to the reader. As evidence of this want of system in the book, we may instance that, between a chapter of the highest scientific value, on the mode of progress of the science of natural parturition, and another, of at least equal value, on the curves of the developed genital passage, is inserted a paper upon long delay of labour after the discharge of liquor amnii, of little importance relatively, and certainly having little logical connexion with the main purport of the work. Again, a paper of no great merit, on the chief directions and extents of uterine shrinking, is placed between two papers of very great importance and originality, viz., those on the efficient powers of parturition, and on the tensile strength of the fresh adult fœtus. Lastly, the author's excellent address on Obstetric Medicine, delivered at the meeting of the British Medical Association in 1873, is appended to this work, for reasons that are not obvious, considering the very slender bearing it has upon the mechanism of parturition.

These blemishes, however, are but like spots on the sun compared with the intrinsic merits of the work, and we are only sorry that those slight defects of form exist, as they are not unlikely to deter the superficial reader from entering into the scientific treasures contained in the volume.

But to briefly consider some of the leading papers.

In the paper on the curves of the developed genital passage, the author, while maintaining the ordinary view that the axis of the impregnated uterus in the case of a well-formed pelvis coincides with the axis of the brim, combats ably, and, we think, most successfully the recent views of Schultze and Schatz, that the uterus has normally a posterior obliquity, and rejects the somewhat absurd practice, founded on this theory by Schultze, of placing a pillow below the loins of the parturient woman during the latter part of the second stage to facilitate the labour. If such an attitude facilitated labour, it is very strange, as our author suggests, among other very many and cogent reasons, that instinct directs a woman, straining in labour, to adopt an attitude nearly the opposite of that implied by extension of the spine, viz., one of a certain degree of flexion.

In connexion with this subject, we may note that upon the fact of the existence almost universally of lateral obliquity of the uterus, Dr Duncan elaborates a beautiful and original theory of the mode of production of presentation of the face. This is the subject of Chapter XIV., but may be considered along with Chapter III.

According to Dr Duncan, face cases most frequently arise in consequence of the uterus being usually bent to the right side or the left—most commonly to the right. This bending of the uterine axis converts the developed lower segment of the uterus into a bent tube. That aspect of this tube corresponding to the side towards which the organ is inclined, being necessarily convex, consequently presents relatively greater resistance to a body passing over it than the opposite aspect, whose surface is concave. To understand how a face case arises according to Dr Duncan's theory, let us now suppose we have a case of right lateral obliquity of the uterus, with the fœtus presenting the vertex occiput to the right. It is plain that when the uterus begins to press the head downwards, there is a tendency for the occiput to be retarded by the relatively greater frictional resistance presented by the convex surface of the lower part of the uterus on the right, whilst descent of the forehead is facilitated by the relatively less obstruction it meets with as it passes over the concave left side, and thus there arises a tendency for the head to become extended on the trunk so as to convert the vertex presentation first into a brow, and ultimately into a face. Suppose, in addition to this, that we have a degree of dolichocephalus present, which Hecker has shown to be very frequently the case in the heads of children presenting by the face, and we see that the long posterior lever would act in such a manner as to aggravate the existing tendency of the occiput to be retarded.

This theory, which is at once very much more clear than the mystifications of Schatz on this subject, and much more definite than the loose and painfully general views lately enunciated by Ahlfield, explains the previously observed fact, that face cases, with the chin to the left, occur more frequently than they ought to happen were they simply the result of extension of cases of occiput to the right. For in the other fact, that the lateral obliquity is so much more frequently to the right than to the left, Dr Duncan's theory finds an easy and consistent explanation of the above-mentioned circumstance, viz., because that the occiput experiences relatively much more frequently obstruction to its descent when it is towards the right, than when it is towards the left. Again, the relative rarity of presentations of the face in British, compared with German practice, is explained by Dr Duncan, by the fact that the left lateral position, which is that usually adopted in British practice, undoes right lateral obliquity of the uterus, and consequently removes the great cause of face presentation, whilst the dorsal decubitus, usually adopted in Germany, does not do so.

It is interesting to observe, as Dr Duncan points out, that Baudelocque entertained this idea of the origin of face presentations, though his less accurate knowledge in regard to uterine obliquity prevented him from stating the case with the same precision as Duncan, who, however, was led to his present opinion by independent and original investigation, before he saw Baudelocque's statement.

Chapter VII. contains an able refutation of the wild views enunciated by Haughton some years ago, relative to the amount of the force which he holds a woman may exert through the volun tary abdominal muscles. Our author's views on this subje cannot fail, we feel sure, to find acceptance with every practical accoucheur.

The article on uterine shrinking is original and thoughtful, but its importance is not very evident. The same cannot be said of Dr Duncan's experiments on the tensile strength of the fresh adult fœtus, which is the most perfect contribution which has yet appeared to the solution of the much vexed question as to whether long forceps or turning is the preferable operation in cases of slight narrowing of the pelvic brim. Only experiments, such as Dr Duncan has instituted for the solution of this difficulty, can solve this problem. From these experiments, five in number, made by attaching heavy weights in successively increasing amount to the ankle of the fœtus, whose head was held in an aperture so cut in hard wood as to represent a narrow pelvis, our author comes to the conclusion that,—

1st, The life of the child is compromised, if a force of about 100 pounds is exerted on the neck, as the spinal column was found to give way when weights varying from 90 to 147 lbs. were put on.

2d, That on an average a weight of 120 lbs. will effect decapitation of the fœtus.

3d, That one leg for traction purposes is quite enough, as by one limb you find weights are invariably held in suspense which are sufficient to effect decapitation.

These experiments cannot well be overestimated, as they invest with scientific accuracy problems that have led to endless guessing and to a terrible waste of words and logic; whilst they prove that not much if any greater force need be exerted by traction on the child's body without compromising its life than can be employed with forceps.

With the author's views in regard to Kyneke's doctrine of the synclitic motion of the fœtal head we entirely agree, and even think that Dr Duncan wastes his powers in bestowing so much attention upon the refutation of a view so plainly incorrect. Kyneke's synclitic motion of the fœtal head we hold to be much of a piece with his extraordinary observations on the opening of the cervix uteri during labour from below upwards, instead of from above downwards, as it is found to dilate in the hands of all other observers we have yet heard record what they have seen. Kyneke we hold to be a very ingenious writer, but we cannot help feeling he is mentally better fitted for being a poet than an accoucheur, because his imagination is so apt to run away with his judgment. We may well forgive him, however, for being the occasion of inducing the author to communicate to the British profession the exceedingly interesting observations of Dohrn, Fankhauser, and other German authors upon the shearing (verschiebung) of the fœtal head under the influence of strong pains during the second stage. These remarks occur in the course of Dr Duncan's paper on the synclitic movement of the fœtal head; they are beautifully illustrated by two engravings, copied from Fankhauser's original paper, and altogether deserve the careful study of every scientific obstetrician.

In respect to his views and deductions therefrom regarding the mechanism of the third stage of labour, or the mode in which the placenta is expelled, which is the subject of Chapter XVI., we may state that we entirely and absolutely agree with Dr Duncan, and that, too, after extensively testing the truth of his opinions. We have found that invariably the edge of the placenta presents at the os, if the natural mechanism is left to act undisturbed. We have also seen smart hæmorrhage and other troubles proceed from traction on the cord and consequent derangement of nature's mechanism.

The publication of the supplementary part of Braune of Leipzig's Homolographic Atlas is the occasion of Chapter XVIII., which contains a short but good description of the changes undergone by the cervix uteri during labour, supported specially by reference to Braune's plates.

We have left ourselves space merely to name the chapters on the increased length of the cervix uteri after labour, and on the production of inverted uterus, and must pass on to the last and certainly not least important subject treated at large in the work, viz., the chapters on Placenta Prævia.

This is the subject of five chapters, XXI.–XXV. inclusive, which embrace observations on—

"*1st*, Hæmorrhage during pregnancy in cases of placenta prævia.

"*2d*, The spontaneous separation of the placenta when it is prævia.

"*3d*, The causes of unavoidable hæmorrhage during miscarriage or labour when the placenta is prævia.

"*4th*, The sources of hæmorrhage during miscarriage or labour at full term in cases of placenta prævia.

"*5th*, The mechanism of arrestment of hæmorrhage in cases of placenta prævia."

We have thus in this part of the work a real and very successful attempt at systematizing our knowledge on the mechanism of this department of morbid parturition.

Our author holds that there is no real distinction between accidental and unavoidable hæmorrhage, but that hæmorrhage during pregnancy in cases of placenta prævia, before the onset of labour pains, is really accidental, and occurs so frequently in cases of placenta prævia merely because the position of the placenta over the lower segment of the uterus favours accidental bleeding. He enumerates the following four ways in which he believes such hæmorrhage may occur during pregnancy:—

"*1st*, By rupture of a utero-placental vessel at or above the os uteri.

"*2d*, By rupture of a marginal-utero-placental sinus within the area of spontaneous premature detachment, when the placenta is inserted not centrally, or covering the internal os, but with a margin at or near the internal os.

"*3d*, By partial separation of the placenta from accidental causes, such as a jerk or fall.

"*4th*, By partial separation of the placenta, the consequence of uterine pains producing a small amount of dilatation of the internal os. Such cases may be otherwise described as instances of miscarriage commencing, but arrested at a very early stage."

The most interesting and original part of the author's handling of this subject is, however, his paper on the spontaneous separation of the placenta when it is prævia.

The author, rejecting the old view that separation of the placenta is due to development of the cervix during pregnancy or labour, for the very good reason that the placenta is never, and can never be, attached to the cervix—and also the view that the separation is due to uterine contraction of the placental site, inasmuch as if that were the case, every labour ought to be accompanied by spontaneous separation of the placenta during the first and second stages, since there is no evidence to show, or ground to believe, that the uterine contractions do not diminish the uterine area to an equal amount in all its parts—refers it to the expansion which is necessary to convert the lower portion of the body of the uterus into a tube for the transmission of the fœtus. We give here the author's views in his own words. At page 345, Dr Duncan says:—

"Expansion of the placental insertion in the first stage of labour is the distinguishing specialty of placenta prævia, and has detachment as its distinguishingly peculiar result. This kind of expansion is only rarely a cause of detachment under any other circumstances. Hydramnios may produce placenta membranacea, and is liable to produce detachment by uterine expansion. In placenta prævia, expansion of the lower part of the body of the uterus in early labour regularly produces, first, stretching of cotyledons and expansion of placental surface, and then separation. While detaching expansion of the uterine site of a prævious placenta, or of a prævious portion of a placenta, is going on in directions transverse to the axis of the uterus, shrinking is going on in a direction parallel to the uterine axis; and this contraction pushes the detached placenta or detached portion into the developing or expanding and elongating cervix, and in the early part of the first stage brings it within easier reach of the examining finger, while at the same time the seat of former placental attachment is getting more and more distant from the external uterine orifice and the examining finger."

We think, in reference to this subject, that there can be no doubt but our author is right in attributing the separation of the placenta to expansion of the lower segment of the uterus, and that separation of the placenta in placenta prævia does not and cannot take place as a result of contraction of the placental site in all directions, as is the case in separation of the placenta after expulsion of the fœtus. But we think he makes too much of the ultimate question as to whether the placental area as a whole is contracted or expanded. Looking at the homolographic sections of Braune, it seems to us that there can be little doubt but that ultimately the placental area is contracted absolutely, for the stretching of the dilated, thinned, and distended cervix is, we conceive, so great as to allow of this; and this, we think, supports the author's views very much in reference to the effect of longitudinal contraction, or, as he calls it, shrinking of the bared placental area, as a means of arresting hæmorrhage. Had Braune's plates appeared before the original publication of this chapter, we think it would have to some extent modified the author's views, and made him think less about the lateral expansion of the placental site. The separation, after all, seems to us to be due more to the development and stretching of the cervix, by which the placental site is allowed to move upwards, under the contraction of the uterus, away from the placenta, than from any absolute expansion of the placental area. It would form a good mathematical problem, and we think, too, a possible one, seeing that we have Braune's homolographic sections to guide us, to determine whether the hemispheroidal lower uterine segment within the spontaneously detaching area is or is not greater in area than the same segment when developed into a cylindrical segment of the uterus, after the limit of complete dilatation is reached. This, being determined with certainty, ought to be an important step towards settling the question whether the placental area is ultimately contracted or expanded on the whole.

One result of Dr Duncan's researches into this troubled question cannot, we think, be overestimated, on account of its valuable practical importance. We mean the definite manner in which he conditions and determines the amount of the spontaneously-detaching area. One need not have read much of the literature of this accident to notice that a great deal of uncertainty has possessed the minds of authors on this point. And if writers who profess to teach the profession have been at sea on the subject, the practical accoucheur cannot be expected to have been guided by clearer ideas. Much good service had already been done by Barnes in this direction, but then his ideas about zones and so forth were far too indefinite to be practically useful.

Our author, however, by a process of close and irresistible reasoning, having first shown that spontaneous detachment must cease in ordinary cases so soon as the dilated inferior segment has reached a diameter of four inches, applies measurement to determine how far we have to pass along a meridian of the uterus, measuring from the inner os, ere such a diameter is reached, and thus accurately determines the extreme limit of spontaneous detachment.

The following are Dr Duncan's chief statements regarding this matter (page 349):—

"With these principles in view, I have examined the shape of the lower uterine hemispheroid in actual or authentically depicted uteri, and I find that a meridian leaves the vertex or centre of the internal os in a direction nearly at right angles to the uterine axis, and that after it has described an arc of one and a half to two inches in length, it becomes nearly parallel to it. At about two and a half inches from the vertex the diameter of the uterine cavity is four inches, and this is about one and a half inch above the vertex, measuring along the uterine axis. A canal of about four inches in diameter is large enough to transmit the fœtus. There is therefore no need for expansion to any considerable amount above that circle of latitude, which is distant two and a half inches, measured along a meridian, from the centre of the internal os. The measurement will of course be less than two and a half inches, if taken from the edge of the internal os instead of its centre. It is thus seen that the area of the lower uterine hemispheroid, over which dilatation of the internal os necessarily involves great expansion of uterine surface, nearly corresponds with the extent of area which must be expanded in order to transmit the fœtus. Expansion beyond a diameter of four inches would ensure very slight extension of uterine surface, and a consequent slight detaching power—so slight, indeed, as to be probably more than counterbalanced by placental expansibility. I therefore conclude that the detaching area of the lower uterine hemispheroid, covering a portion whose vertex is the centre of the internal os uteri, has a meridian semi-length of about two and a half inches, and that its chord measures about four inches."

No practitioner need now be in serious doubt as to when he may expect separation and consequent bleeding to cease of themselves in a case of placenta prævia. There is thereby effected a precision hitherto unknown in the solution of the practical questions as to when an operation is necessary, and at what time it ought to be undertaken in such cases.

In regard to the arrest of hæmorrhage in placenta prævia, Dr Duncan refers it chiefly to shrinking of the placental area, and insists also considerably more than is usual upon the effects of changes in the degree of intra-abdominal pressure in causing or retarding such hæmorrhage, and finishes up with a condemnatory criticism of the views of Gooch, concerning the occurrence of hæmorrhage although the uterus be firmly contracted. In this we feel certain he is right. Very many other points in these and the other papers deserve to be noticed, but space will not permit.

We must say, in conclusion, that we have read the book with pleasure and satisfaction, and regard it as a valuable contribution towards placing obstetrics upon an exact and scientific basis. It is the work of a physician whose mind is replete with knowledge both scientific and practical, to whom the obstetrical profession, already very largely his debtor, is thus laid under fresh obligations.

It ought to be in the library of—and, what is more important, ought to be diligently read and re-read by—every accoucheur who wishes to see the science of his department built up on true principles and more and more approximated to an exact science. We cordially recommend the work to the attention of our readers.

Heredity and Hybridism: A Suggestion. By EDWARD W. COX, S.L., Author of "What am I, or a Popular Introduction to Mental Philosophy and Psychology," etc. London: Longman and Co.: 1875.

WE are sorry to learn that in publishing his "Suggestion," Serjeant Cox anticipates that he will be "scared by abuse, and put upon the moral rack;" and although the thumb-screw and the stake are for the present out of fashion, the Serjeant doubts how long they will continue to be so "in the present state of religious rancours." We have not found Mr Cox's work a production likely to bring upon him the sufferings or honours of martyrdom. He sets himself to the task of explaining what appears to be a waste of power in nature in the requirement of two parents for that which might be more readily produced by one. At first sight, the Serjeant is disposed to think with a celebrated Roman that the existence of two sexes is something clumsy and needless. What is the use of sperm-cell and germ-cell, when their functions might have been performed within one cell-wall? But if we adopt his theory, nature, as the learned Serjeant brings out, is not guilty of a clumsy and needless contrivance. Every living being has two parents; but then every living being has two sides; every living being is composed of two halves, more or less symmetrical, joined together at the middle line. Mr Cox thinks that every organism is produced from two germs, one furnished by the father, and the other by the mother. "With animals, the nerve system expands and grows, and builds the body about itself," and with plants it grows with some other "formative force." These germs cannot be seen, even with the aid of the most powerful instruments, but they have a real morphological existence, and can be demonstrated by reasoning like the hooked atoms of Epicurus. As the male furnishes one germ, and the female another, this explains why the offspring has the qualities of both, and many other difficult questions of hereditary peculiarity and hybridism. The learned Serjeant does not explain, if the mother furnishes one half and the father one half, and their joint product is connected in the middle line, why one half the body is not like the father, and the other like the mother. Ought we not to expect, for example, that when a white man marries a black woman, the result should be a child white on the one side and black on the other? He, however, is disposed to think that something of this sort may actually be the case, for he suggests, "it would be a curious subject for inquiry, by persons who have leisure and opportunity, how far, in respect of strength and similarity of shape, each of the two sides of the body resembles the father or the mother."

If the Serjeant's theory were discussed at greater length, time and space might be occupied which in a medical journal could be better taken up by other subjects; but we may observe, in conclusion, that it is difficult to see how nature can be exculpated, even by so subtle an advocate, from requiring two things to accomplish what might be the work of one. Physiologists ask for two cells, a sperm-cell and a germ-cell, and Serjeant Cox asks for two germs. In either case, you have the figure 2. But then, says the Serjeant, you have a double set of organs. But why have we a double set of organs when one set would do the work? Here we come to a standstill. The learned Serjeant is an acute dialectician, and as people nowadays are fond of speculating on the origin of things, his theory may excite some discussion amongst our modern Epicureans.

Heredity: A Psychological Study of its Phenomena, Laws, Causes, and Consequences. From the French of TH. RIBOT, Author of "Contemporary English Psychology." London: Henry S. King and Co.: 1875.

M. RIBOT has read all the separate treatises that have been published on the subject in French, English, and German, and looked over most of the articles in the contemporary periodicals, and bringing to the work a clear intellect and a respectable amount of learning, he has made a very readable book. Naturally, many of his facts have already done service in other works on the subject. But he has managed to make his book more complete than any previous publication which we have read. The views of Darwin and Herbert Spencer are of course introduced and discussed; but M. Ribot does not seem inclined to give any definite adhesion to their theories. If he has not collected the facts he uses, M. Ribot has at least thought carefully upon them. We have a great many statements made, and a great many questions asked, but the conclusions seem very doubtful. Few observing people will deny that many features are transmissible from the parent to the child; children are like their father or their mother, or resemble both; or sometimes they resemble neither; but it is impossible to predict beforehand whether the particular child will resemble his parents, or in what manner he will do so.

Strength of body, comeliness, and longevity are often hereditary, as are weakness, scrofula, or insanity, but they are not always so. When we come to the hereditary transmission of mental qualities, the vagueness of our knowledge becomes greater. The children now and then have some of the distinctive mental and moral qualities of their parents; often they have none of them, or only one of them: and this is really all we know; we cannot give the how or why.

A breeder of horses or dogs or fowls, confidently expects to get superior animals of the kind he desires. Human beings cannot be experimented on in this way, but we have made a large number of observations, and these have not given us any results which enable us to predict consequences. If we assume that attempts to found hereditary families have had any serious purpose beyond conferring privileges on their descendants, they have proved utter failures; and the hereditary castes of India are composed of very poor craftsmen. M. Ribot's attempts to prove the hereditary character of all kinds of genius form the weakest part of the volume. Some of his proofs or illustrations have no scientific value. For example, in the chapter on the Heredity of the Imagination, he states that upwards of twenty out of fifty poets had illustrious relatives. Among these is Burns, who, we are told, appears to have inherited from his mother that excessive sensibility which made him one of the first poets of Britain. Schiller's mother was a remarkable woman. Racine's son was a good verse-maker. Byron's daughter is put down as an illustrious relative from her talents for mathematics. Goethe took his physical constitution from his father, but his character from his mother. Goethe's children were stupid, because their mother was a woman of low rank and of no great mental vigour. Cromwell married a woman of gentle disposition, on which account "his male issue were Arcadian shepherds, his daughters more fanatical than himself." We have no time to go through facts like these, which can be turned this way or that way with the greatest ease. If there were no better matter than this in M. Ribot's book, we should say that it was only fit to be read to the British Association, or some other quasi-scientific body. We are ready to admit that talent runs in families, at least it sometimes does; but it does not seem to be the highest talent, and when this admission is made, we can say no more.

But some of the facts which M. Ribot brings forward are of real interest. His proofs of the hereditary character of a genius for music are very striking. The book has a completeness about its form which somewhat contrasts with the vagueness and scantiness of our knowledge on a subject in which we have many concrete facts, and few generalizations or scientific explanations. We have chapters upon the moral consequences of Heredity, treating of Heredity and Liberty, Heredity and Education, upon the part played by Heredity in the Genesis of Instincts and of Intelligence. As may be supposed, these chapters are full of difficult and subtle questions, which are ably handled by the author.

The subject, however, is too interesting and important to be lost sight of; and M. Ribot's book tells the reader as much as is known about it, and perhaps a little more.

We have not seen the original, but the translation seems to be well done. It reads smoothly, and is free from Gallicisms. Altogether, we can recommend this work to those interested in heredity.

A Practical Treatise on the Surgical Diseases of the Genito-Urinary Organs, including Syphilis. Designed as a Manual for Students and Practitioners. With Engravings and Cases. By W. H. VAN BUREN, A.M., M.D., Professor of the Principles of Surgery, with Diseases of the Genito-Urinary System, and Clinical Surgery, in Bellevue Hospital Medical College, etc.; and E. L. KEYES, A.M., M.D., Professor of Dermatology in Bellevue Hospital Medical College, etc. London: J. and A. Churchill: 1874. Pp. 666.

THIS is at once a difficult and a pleasant book to review—difficult, because the authors have taken so much care to make it at once concise and exhaustive, that there is not much that can be extracted or given in abstract; pleasant, because there is so little to find fault with, and so much to praise. The authors speak from the result and outcome of much experience carefully weighed and accurately digested. There is little evidence of idiosyncrasy, petulant self-consciousness, or egotism in the work of either of them. Much evidence of an honest desire to instruct, and of a practice and theory each guided by and subordinated to common sense.

The copious material of the work is so well arranged, and so liberally indexed, as to be thoroughly under command for reference; the practical details of treatment are precise, without being wearisome in detail; the cases are accurately recorded without prolixity; and the illustrations, if rough, are practically useful to the student.

Professor Van Buren's portion, which occupies fully two-thirds of the book, is to our mind more satisfactory than that of Professor Keyes. This is owing to the nature of the subject, for the preciseness and accuracy of our knowledge of diseases and injuries of the bladder and urethra, is in pleasing contrast to the vagueness and doubt with which theories of syphilis and its varieties are still beclouded.

We had marked many passages for quotation or remark, but space forbids, and quotation is hardly needed from a work which will doubtless have a large circulation.

The varieties of so-called gonorrhœa are well described (pp. 55-58). Extravasation of urine and its consequences receive more attention in a scientific or experimental point of view than in most works (pp. 143-145). Some very interesting experiments originated by Dr Van Buren are detailed, in which on many occasions large doses, 30 to 60 minims, of healthy urine were subcutaneously injected in healthy patients, instead of morphia. In all of these absorption was perfect; in none, did abscess or local death of tissue follow the injection.

Stricture and stone are both treated practically, concisely, and sensibly, and in both the treatment in America seems to be very similar to our own, with the exception that a greater preference is shown for soft bougies in the earlier stages of the treatment of stricture than is usual in this country.

The chapter on Varicocele is excellent. In its treatment Dr Van Buren says, "No operation offers a fair prospect of relief without serious accompanying risks, except excision of the scrotum" (p. 471).

We cannot give details of many interesting points in the description of syphilis. We conclude this brief notice by referring to an important paragraph, containing instances of rapid absorption of the syphilitic virus, in one of which a small abrasion cauterized freely with nitric acid within twelve hours of the infection, still was followed by manifestations of constitutional syphilis (pp. 508-9).

We would recommend this work in high terms of praise to surgeons wishing a full account of injuries and diseases of the genito-urinary organs in a single volume, and in an easily accessible form.

Syllabus of Materia Medica, for the Use of Teachers and Students. Based on the Relative Values, and on a Selection of the Articles and Preparations in the British Pharmacopœia. Second Edition, including the "Additions" in Reprint (1874) of British Pharmacopœia. By ALEXANDER HARVEY, M.D., Professor of Materia Medica in the University of Aberdeen; and ALEXANDER DYCE DAVIDSON, M.D., Assistant-Professor. Pp. 17 and 45. London: H. K. Lewis: 1874.

THE title-page of this little book so far exhibits the purpose it is intended to serve. It is, in fact, an attempt to give practical effect in the branch to which it refers to the principle of a "Definition and Limitation of the Areas of Instruction and Examination,"—a principle first pointedly brought into view some years ago by the *General Medical Council*, and on which, in as far as relates to the *examination* of candidates, and in respect of four branches—to wit, Botany, Zoology, Chemistry, and Materia Medica—the Council has last year put its *imprimatur.*

Our review of this most admirable little work has been far too long delayed; for to review it aright, the great question of medical education in relation to future examination on the one hand, and future usefulness in practical work on the other, should be discussed.

Limitation of the area on which examinations are to be conducted, seems really now to be necessary, from the extraordinary increase in bulk and extent, as well as in exactness, of the knowledge we possess, not only in practical subjects required in medical study, but in the allied sciences which the student has to master. The dedication or preface to this little work to the President and Members of the General Medical Council discusses the whole subject with great pith and power, and deserves careful perusal.

In the work itself, in very small compass, and with admirable arrangement, we find a limitation of the area of study of materia medica by, (1) An estimate of the relative values of the articles and preparations in the Pharmacopœia, and (2) A selection of the more important of these. This is done in the most careful way, and with such aids of type, numerals, etc., as to strike the eye, and command the attention even of the most desultory student.

We recommend the work most cordially to the notice of every teacher and student of materia medica who can read the English language. It is a model to be followed by teachers in other departments.

Dental Pathology and Surgery. By S. J. A. SALTER, M.B., F.R.S. London: Longmans and Co.: 1874. 8vo.

De l'Evolution Pathologique de la Dent de Sagesse. Par M. V. PIETKIEWICZ. Gaz. des Hopitaux, Novembre 1874.

WITHIN the last quarter of a century our acquaintance with the pathology and treatment of dental disease has wonderfully increased. Many erroneous theories have been exploded,—much that never was dreamt of has been revealed, and an amount of medical and surgical knowledge expected of its practitioners that entitles dentistry to a very different position from what it occupied prior to that time. Nowadays a considerable number of those who used to be courteously denominated *surgeon*-dentists really are surgeons; some of them, such as the author of the work before us, deservedly standing high in the ranks of medical science. When such men devote themselves to writing upon a departmental section of surgery like that under notice, the result is at once apparent, not only in the grasp and thorough knowledge of the subject displayed, but in the ease and precision with which its phenomena are linked with, or referred to, its surroundings in the animal system as a whole. It is here that the mere specialist is distinguishable from the fully qualified practitioner. The one must seek for the disease, and its cause, and its cure, within the limits of his own province; the other sees its origin and its relief lying in some region which, if not altogether a *terra incognita* to his neighbour, is at least a territory where it would be dangerous to venture too far, and on which, if a wise and modest man, he will not encroach.

Mr Salter's work consists of an octavo volume of some 400 pages, interspersed with no fewer than 133 illustrations. While, perhaps, the greater portion of the treatise may more immediately interest and concern the dentist, there is much to attract the attention of the surgeon in the broader aspects of practice. Such are the chapters on odontomes, on gum tumours, on dentigerous cysts, and jaw necrosis. Many of the morphological changes, commencing in or confined to teeth, frequently excite little notice until they have acquired an extent and character apt to lead to errors of diagnosis.

But even at this late stage, an acquaintance with, and recognition of, what might be considered unimportant circumstances sometimes saves a patient from an unnecessarily severe operation. Thus, in speaking of dentigerous cysts, Mr Salter reminds us that "one of the most usual symptoms, which is also an important *diagnostic sign*, is the absence from the mouth of some tooth or teeth, which should have appeared, and which have never been extracted." . . . "In some instances, considerable portions of the jaw-bone—even as much as half the inferior maxilla, have been removed for dentigerous cysts. In all these cases, I believe," says he, "the operation was undertaken from an incorrect diagnosis. A knowledge of the real nature of the disease and an early interference would have rendered such proceedings quite unnecessary."

An interesting chapter is devoted to the discussion of "Phosphorus disease" or maxillary necrosis from the fumes of phosphorus. Here the author strongly upholds the view that dental caries must exist during exposure to the phosphorus fumes in order to such consequences as maxillary necrosis being produced. A little haziness, however, mingles with this theory when, in the chapters following, he classes it with the occasional action of fever poisons on various organs in the patient's body. In whatever manner the fumes of phosphorus may act, they must always be considered among inorganic agencies; while the nature of a fever poison, on the other hand, must be held as acting through the medium of its vital endowments, and, so far as its secondary local effects—such as jaw necrosis, cancrum oris, or other affections of the mouth—are concerned, only acting in the same manner as when gangrene of the extremities is so produced; the poison, if poison it be, acting from within, and confined in such secondary effects to the patient in whom it has been developed. There seems no doubt that the influence of phosphorus fumes is the cause of the terrible malady attributed to it, and that their action is a local one. Lorinser, who first wrote upon this subject so early as 1839, Strohl, Heyfelder, Roussel, Gendrin, Sédillot, Von Bibra, Geist, and Ebel, all adopt this opinion in a general manner. The poison, according to them, acts locally through the saliva, or by direct contact, or in both ways. Drs Jüngkeen and Helfft supposed the disease to be a merely accidental complication occurring in the debilitated condition of those exposed to the fumes, the cold, the moisture, and other incidental vicissitudes of match-making; while Martius of Erlangen, and Dupasquier of Lyons, ascribed it to arsenic, as an impurity of the phosphorus employed. All these opinions have, however, long been shown to be easy of refutation. And although Dr Bristowe, in his report before the Privy Council in 1863 on the manufactories where phosphorus was employed, disputed the fact of carious teeth necessarily pre-existing in order that the disease could be produced, he did so merely upon the statement of patients upon a point which Mr Salter justly observes as one requiring a particular and special knowledge and habit of search to discover, and one which would be excusably overlooked by any ordinary practitioner of medicine or surgery. We may therefore assume it as established that the primary and essential cause of this form of maxillary necrosis is the direct application of oxidized phosphorus to the exposed tooth pulp.

In his remarks on jaw necrosis after eruptive fevers, the author adduces a number of interesting and instructive cases, from which he concludes that, the teeth being members of the dermal system, we should naturally expect them to share the effects of such fever poisons as spend their chief force upon the skin. And it is here, at p. 310 of his book, that he makes the statement, the correctness of which we have already called in question—"that the necrosis and exfoliation of bone and teeth after the eruptive fevers is essentially the same as the maxillary necrosis in the victims of phosphorus fumes, and that it is the result of the local application of a specific poison to the vascular parts of the teeth." But there is much yet to be worked out in the pathology of maxillary necrosis, from whatever source it may arise. The late Professor Syme used to describe, in his clinical lectures, a condition of the jaw and superjacent gum where, although a probe could be passed down to the bone in various spots, it was not necrosed, and his mode of cure was to strip the gum from the alveolar surface, and probably with it a part of the periosteum—the result being that exfoliation of a portion of the weak bone was induced, and a rapid recovery of the patient in every case ensured.

Dental caries is a subject almost unavoidably entered upon in a work of this kind, and we are glad to find Mr Salter leaning towards, if not wholly supporting, the view of its being a vital process, rather than one of mere chemical decomposition, as is upheld by some authorities even at the present day. Some interesting observations are brought forward by him on the process of decay as seen in the walrus and hippopotamus tooth-bone blocks formerly used in the manufacture of artificial teeth. Such specimens, however, can be compared with true dental caries only after the dentine is devitalized by previous disease. He may not be aware of the subject being discussed in the pages of this Journal somewhere about twenty years ago.

The serious consequences sometimes resulting from abnormal development or evolution of the third molar or wisdom tooth is largely alluded to by Mr Salter, and forms the whole subject of M. Pietkiewicz's paper. Epilepsy, delirium, paralysis, and diffuse burrowing suppuration have been recorded as proceeding from this source. In the major work under review, the author writes as follows:—"The French surgeons have been particularly alive to the interest of such cases, and have published valuable contributions to their illustration. Velpeau delivered an interesting lecture on this subject in the hospital of La Charité, a translation of which appeared in the Journal of the Provincial Medical Association for 1841. . . . . Another and more recent lecture on the same subject has been published by M. Nélaton, in the Journal de Médecine et de Chirurgie Pratique, Fevrier 1862, entitled 'L'Accidents produits par l'Éruption des Dents de Sagesse.'" He also adduces the memoir of Toirac as the first writer on this subject, and the cases of M. Nélaton where the symptoms produced were mistaken for caries of the jaw, syphilis, and cancer.

A chapter on Cleft Palate closes Mr Salter's excellent and instructive volume. Nothing very new or original is cited in reference to this distressing imperfection. The avoidance of everything tending to *enlarge* the fissure, such as the fitting into it of indiarubber vela, is strongly urged. All obturators of the kind he recommends to lie *upon*, not *within*, the opening. A paper bearing on such points was, not many years ago, read before the Royal Society of Edinburgh, and quoted in some of the works on such matters recently published in London, showing that not only does the avoidance of introducing foreign substances between the edges of the cleft prevent its enlargement, but that the natural tendency of this lesion, if not interfered with, is of itself to diminish in width as the patient grows up. Neither this paper nor the theory inculcated in it, however, seems to have come under Mr Salter's notice as strengthening the position he takes up on this particular topic.

In conclusion, we can only reiterate that the book is, from beginning to end, written in a scientific spirit, and is an interesting and valuable addition to our literature of the subject of which it treats in so able and practical a manner.

On the Curative Effects of Baths and Waters: Being a Handbook to the Spas of Europe. By Dr JULIUS BRAUN, Spa Physician, Rehme-Oeynhausen. Including a Chapter on the *Treatment of Phthisis by Baths and Climate*, by Dr ROHDEN of Lippspringe. An Abridged Translation, with Notes, edited by HERMANN WEBER, M.D., F.R.C.P., Physician to the German Hospital. London: Smith, Elder, and Co.: 1875.

DR BRAUN'S work, on its first appearance in Germany a few years ago, created quite a sensation among balneologists. It was searching and sceptical, and perhaps too subversive of received opinions. Still, it was hailed as a vigorous attempt to place the principles of balneological practice on a scientific basis. We have to thank Dr Weber for presenting the book to us in an English dress, and for having enriched it with notes of his own, referring chiefly to climatic health resorts. The book derives something of a special character, owing to more than one sixth part of it being occupied by Dr Rohden's monograph on the Treatment of Phthisis.

It would be easy to offer various criticisms on it—for instance, it is questionable whether it would not have been better to have given the mineral contents of the waters in the 10,000 parts rather than in the 16 oz., according to the old fashion: whether, also, in a work of this size, a few tables, giving the whole instead of only the chief contents of many of the principal waters, might not have been introduced: whether, too, the editor has done well in inserting the names of a certain number of local practitioners, Dr Braun having thought it better to omit all names in his third edition. In an ordinary guide-book, no doubt, it is convenient to find the names of doctors as well as of hotels, but this book aspires to a higher position.

As the arrangement of the work renders it necessary to make several references to the index, in order to get a tolerably complete account of any particular station, the volume will scarcely fulfil its purpose as a convenient Handbook to the Spas of Europe, in spite of its excellences in other respects. Nevertheless, we commend it to the profession, as one that will help to make the principles of balneology better understood by English practitioners.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING V.

Wednesday, 3d March.—Dr D. R. HALDANE, *President, in the Chair.*

Exhibition of Patient:—

I. *Mr Joseph Bell* showed a patient, G. K., whose case, along with some others, he had brought before the Society [*vide* Journal for September 1874, p. 216]. The formidable wound, of which they now hardly saw the cicatrix, had been accidentally inflicted with a very sharp scythe; and extended from the lower edge of one sternomastoid obliquely across the neck to the upper end of the other sternomastoid, notching the anterior edges of both. In this way a large triangular flap was reflected downwards. Both the carotids escaped, but the larynx was cut into, the thyroid cartilage being divided into four pieces; that is, it was split mesially, and each half divided into two parts. The vocal cords, however, escaped injury. Mr Bell, along with Drs Thomson and Lucas of Dalkeith, saw him shortly afterwards. He first stopped all bleeding, and then performed tracheotomy. He then carefully stitched up the larynx by means of catgut sutures put through in all directions, but not tied until all had been passed. The patient made an uninterruptedly good recovery. On the ninth day the tube was removed, and he was walking about within three weeks. Some months after he spat up a small portion of necrosed bone, probably from the hyoid, but he could now speak distinctly though rather hoarsely. The hoarseness, however, was slowly diminishing. Mr Bell finally drew attention to the importance of performing tracheotomy in such cases; and also to the fact that the case confirmed Roser's observations, viz., that in all its course the common carotid was protected by the sternomastoid, and not at any point subcutaneous. This he had frequently confirmed on the dead subject, by passing needles at the anterior edges of the sternomastoid, and showing that the carotids could not be thus wounded.

Exhibition of Pathological Specimens, etc.:—

II. *Dr P. H. Watson* showed—(1.) A LEG, in which the lower part of the tibia was affected with a giant-celled sarcoma. The disease originated in the periosteum and extended upwards beneath the tendinous sheath, and had incorporated itself with the periosteum of the fibula.

(2.) EXCISION OF THE ELBOW-JOINT, effected after the method of M. Ollier of Lyons.

(3.) EXCISION OF THE ANTERIOR PART OF THE TARSUS AND THE BASE OF THE METATARSUS, in accordance with the method recommended by himself some years ago in a paper read before this Society. The only variation from the method then recommended consisted in the operation being effected sub-periosteally.

(4.) EXCISION OF THE ASTRAGALUS in a case of disease of the bone. The operation was effected by the single J incision upon the outer side of the joint; the astragalus was found anchylosed to the articular surface of the tibia, and required to be severed from it by the use of a keyhole-saw. The inter-osseous ligament, between the astragalus and os calcis, having been divided, the bone was then thrust out of its bed, and removed along with some osseous fragments which remained attached to the ligaments.

(5.) The CARPUS AND BASE OF THE METACARPUS removed by Dr Watson's method. This plan consists in a single linear incision, extending over the lower end of the radius, the carpus, and first bone of the metacarpus, to the ulnar aspect of the extensor tendons of the thumb. The periosteal tissues are then pushed away from the dorsal surfaces of the bones; next, the articulation between the trapezium and trapezoid is opened up, and the dorsal ligaments of the wrist-joint proper having been opened, access is afforded to the flexor aspect of the carpus, so as to pare away with a periosteum elevator the soft parts as far as half an inch beyond the base of the metacarpal bones, after separating the pisiform bone, and cutting across the unciform process of the unciform bone. The bones, as low as exposed, are then cut across. The trapezium is next dissected away from its articulation to the base of the metacarpal bone of the thumb.

Dr Watson stated that in all those operations of excision which he had recently practised, he uniformly stuffs the cavity with chloral lint, and applies a firm compress of the same, and a bandage over all, before removing the Esmarch's band. The operations were thus absolutely bloodless. This first dressing is removed after the lapse of forty-eight hours, and the stuffing renewed day by day, until the cavity is occupied by granulation tissue. That in this way the formation of abscesses and sinuses was prevented, while the period occupied in the healing process was not materially prolonged.

(6.) An UPPER JAW, the interior of which is occupied by epithelioma, successfully removed from an elderly female patient (upwards of seventy years of age), in which the disease commenced in the gum and penetrated the cavity through an alveolus.

(7.) The INSTRUMENTS employed by M. Ollier of Lyons, in his sub-periosteal operations, and which Dr Watson had employed with much satisfaction in the recent excisions presented to the Society.

III. *Dr Thomas Keith* showed TWO FIBRO-CYSTIC TUMOURS OF THE UTERUS, and the photograph of a third, which he had recently removed. In each case, the division was made just above the vagina; all three recovered, and these were, so far as he was aware, the only successful cases of removal of the uterus in Scotland.

IV. *Dr Argyll Robertson* showed—(1.) An EYEBALL with a large tumour attached to its posterior surface, which he had removed in June last, from the left orbit of a woman aged 59. The disease appeared only to have been of nine months' growth, previous to her coming under Dr Robertson's care. When first seen by him, the eye was greatly protruded by a rounded tumour occupying the orbit. The conjunctiva was highly congested and chemosed, and the movements of the globe were impaired. From the rapidity of growth, the tumour was considered of malignant nature; but as the general health of the patient was good, as the bones of the orbit did not appear to be involved in disease, and as there did not appear to be any affection of other organs, with the exception of a slight enlargement of a lymphatic gland near the left angle of the lower jaw, the contents of the orbit were removed by operation. A saturated solution of chloride of zinc was applied to the apex of the orbit to destroy any remains of disease left behind. The patient recovered well from the operation; but after the lapse of three months, the disease was observed by Dr Dodgson of Cockermouth, her ordinary medical attendant, to have returned in the orbit, and also under the skin in many parts of the body, in the form of numerous small tubercles, the most noticeable tubercle being situated near the left nipple. These growths increased, and the patient became gradually more and more exhausted, till she died in the middle of December. On examining the excised eye and tumour, the interior of the eyeball was found to be filled by a soft melanotic tumour, which under the microscope was found to be composed of numerous large-sized oval cells filled with numerous nuclei and pigment granules. The growth that occupied the orbit consisted of a close fibrous network, in the interstices of which small oval and spindle-shaped cells were found containing numerous nuclei.

Two photographs of the patient prior to operation were shown.

(2.) *Dr Robertson* also exhibited an EYE, at the lower margin of the cornea of which a portion of stone might be observed presenting through an ulcerated opening. The stone had penetrated the eye fifteen years previously, but after the inflammation following the injury subsided, the patient had not suffered any irritation from the presence of the foreign body till last summer, when the eye became severely inflamed, and remained irritable till he applied for relief three weeks ago, and Dr Robertson removed the eye. The preparation was kept in solution of chloral, which had answered well as a preservative agent, and had not rendered the cornea any more opaque than it was at the time of removal.

V. *Mr Bell* showed a specimen of EXFOLIATION OF THE WHOLE MUCOUS MEMBRANE OF THE FEMALE BLADDER. In December last, the patient had been delivered of a still-born child by Dr Thatcher. About a month afterwards, Mr Bell was consulted on account of incontinence of urine. At the mouth of the urethra, which was largely dilated, what seemed to be a tumour could be detected. Mr Bell was able to pass his finger up between the bladder and this, all round it, and thus gradually detached it, and so pulled it out. On examination it was found to be the whole mucous membrane of the bladder coated with phosphates. The patient made a good recovery, and has now been able to keep her urine for two hours. There is, however, still a little incontinence if she walks about much. In conclusion, Mr Bell remarked on the great rarity of such cases, and the curious fact of the rapid renewal of the mucous membrane after such a complete removal.

VI. *Dr James Dunsmure*, jun., exhibited A PIECE OF A SPINAL CORD which was taken away at a post-mortem examination of a case of cerebro-spinal meningitis, which died in the Children's Hospital on the 11th February. The preparation showed a layer of inflammatory lymph between the membranes and the true nerve substance, which, when recent, was an eighth of an inch thick. The part had been preserved in a solution of chloral.

VII. *Dr Miller* showed a FLEXIBLE BULBOUS-POINTED CATHETER, which he had got made for him by Mr Young, of a longer pattern than ordinary, for use in cases of prostatic enlargement. Dr Miller recommended the employment of this instrument in preference to the ordinary catheters, which are too short. Dr Miller pointed out that the aperture of these instruments is a long way from the point, and that therefore they require to be made longer than other catheters.

VIII. *Dr Miller* showed a SMALL EPITHELIAL TUMOUUR which he had removed from the lower lip of a female. The woman said that she never had a pipe in her mouth in her life. Dr Miller mentioned this on account of Mr Bell having at last meeting stated as his experience, that those females in whom he had met with this disease were all smokers.

IX. *Dr Miller* also showed a TOE which he had removed the week before. Dr Miller had employed an elastic ligature in order to restrain hæmorrhage. The method was an advantage during the operation, but a disadvantage afterwards. Whenever the ligature was removed, the oozing was excessive, and primary union was completely prevented, by a portion of the flap threatening to slough. A deep sulcus remained for several days where the elastic ligature had been, though it had been by no means tightly applied. Dr Miller did not think Professor Esmarch's method applicable to such cases of operation.

X. *Mr A. Wilson* then read NOTES ON THE GROUSE DISEASE. This paper appears in the present Number of the Journal, page 910.

The President remarked on the interest of the subject. It was one, however, on which he was not inclined to theorize.

XI. *Dr Macdougall of Galashiels* then read a paper on some cases illustrative of the PRINCIPLES OF CONSERVATIVE SURGERY, which will appear in an early Number of the Journal. Two of the patients and a photograph of the third were shown at the conclusion of the paper. In the case of resection of elbow-joint for compound fracture, the patient, in addition to the ordinary movements, could freely *over-extend* the forearm.

The President was much pleased with the paper, as showing what could be done by conservative surgery.

Professor Spence felt sure that the members of the Society must have derived both pleasure and instruction from Dr Macdougall's interesting paper. The case of complete resection of the shaft of the tibia was interesting on account of the reproduction of bone and the after usefulness of the limb. In regard to amputation in such cases, when the patient's life was in danger, he had found, as the result of his experience, that the patient had a better chance of recovery with his limb on, than with it off. To amputate while the patient was suffering from irritative fever, would likely result in death. But by waiting, most patients will eventually recover; and as Dr Macdougall's case showed, by resection this risk of death could be much diminished, a useful limb ultimately got, and all risk and suffering abridged. In regard to the usual bleeding in necrosis cases, he thought that these were the very cases in which the so-called bloodless method will be found specially useful. It prevented all bleeding during the operation; and by plugging the wound with lint without tying any vessels, all after hæmorrhage could be avoided. In such conditions, therefore, it was good. The results of resection of the elbow for injury were satisfactory. As to subperiosteal resection of joints, that point was settled for us in general, as, in the gelatinous form of disease at least, the periosteum was already destroyed. He was satisfied that when an excision of the elbow-joint was done by the ordinary method, especially if bone was removed with sufficient freedom, they got a useful movable joint. Fear of a flail-joint often led to a stiff one. In resection for injury, the subperiosteal method gave good results. In the case of cicatrization after a burn, resection was exceedingly satisfactory. In such a case as Dr Macdougall's, a sore often formed over the olecranon if the cicatrix was allowed to become chronic. Last year he had a patient on whom an operation performed elsewhere for removal of contraction, had done no good. He had resected the elbow-joint, and very free movement had resulted. Altogether, Dr Macdougall's paper was a highly satisfactory one; and they were all indebted to him for the results of his experience.

Dr Watson did not know whether Dr Macdougall or the Society was to be more congratulated on this paper. Dr Macdougall's success in the practice of surgery was a thing quite to be expected. Even as a student, his strong surgical instincts had shown that wherever his lot was cast he would be sure to find surgical practice and obtain satisfactory results. In the first case it was pleasing to see that, in so large a resection, there was no depressed cicatrix, but instead, osseous reproduction more than enough. According to the old rule, in a case where more than four inches of bone had been removed, the result should have been expected to be a little bone at both ends, and an interval of depressed soft parts. The good result was interesting therefore both pathologically and practically. In the second case, the result was admirable; in fact, the boy seemed to have a better joint than before, to judge from its extensive movements. The case, of which a photograph had been shown, was also a good result. Dr W. concluded by saying that he himself personally, and he felt sure the other members of the Society, were indebted to Dr Macdougall for this paper, which he thought possessed an additional interest from the style of its composition and the manner of its delivery.

Mr Chiene was also much interested in the paper. In regard to the first case of acute necrosis, its pathology seemed to him to be an extensive separation of the periosteum, with a tendency to death in the bone, but not actual death. Dr Macdougall had said that the bone in that case was bare, and therefore dead. Some months ago, he had examined transverse sections of such a bone, after amputation had been performed, and found evidence that it was living, although bare. If, in similar cases, proper precautions in opening the primary abscess were taken, there might be no necrosis. He had been watching a case of large abscess over the femur, opened antiseptically. Previous to this, the patient was exhausted, and with a quick pulse; but since the opening, all had gone well. The temperature had not risen more than a degree above normal. A sinus now only remained, but there was no dead bone.

Professor Spence said that acute necrosis was a conventional term. On one occasion, he had been called to the Medical House of the Royal Infirmary to see such a case, following an attack of scarlatina. He made a free incision down to the bone, and, although the bone was bare, yet none came away; and in some other cases there was only an exfoliation from the surface. In Dr Macdougall's first case, the hectic state of the child had to be considered. By excision, Dr Macdougall had saved a useful limb, and in this the value of the case lay. In such a case, he believed that the whole shaft does not die, for even when removed new material could be seen in bone cells left adherent to the periosteum. He wished especially to point out that the term acute necrosis was only a conventional one.

Dr Watson thought that terms were legitimately used so long as they were retained in ordinary works of reference. In these works the term acute necrosis included not only the death of bone, but the process of dying, and the processes involved in its separation and repair. The term necrosis itself was sometimes, as Mr Chiene indicated, a misnomer, for no bone died, certainly none separated. But there was no harm in using this term, so long as all understood what it meant. Even when a considerable portion of the entire thickness of the shaft of a bone had separated, a longitudinal section showed that the medulla frequently retained its vitality. Nay, further, when such an exfoliation is extracted by the bloodless method, we often can recognise that there is a mass of granulation tissue left behind after we extract the dead structure. Even, therefore, where upon the surface the entire circumference of the shaft of a long bone may appear to be dead, because bare from denudation of its soft parts, neither superficially nor deeply is it safe to conclude that the whole exposed bone has lost its vitality and must separate, any more than to believe that a bone similarly denuded by mechanical means, as by an injury, has ceased to live. As to the effects of certain mystical methods of treatment in favouring repair in such cases, and securing what has been paraded as the absorption of dead bone by the granulation tissue, the whole thing lay in a nutshell. If a piece of bone was dead, it would remain dead, despite any treatment; and if not dead, it could be preserved alive without any mysterious performances.

Mr Chiene said that though he had not the extensive experience which Dr Watson possessed, yet to him the fact of such a large abscess being opened without any resulting irritative fever, was a most instructive fact.

Dr Watson remarked that the opening of large abscesses, if efficiently practised, tended in itself to remove irritative fever. He might further remark, that in all Dr Macdougall's cases his gratifying success had been attained without the employment of any of these so-called antiseptic methods of treatment, so much the fashion of the hour.

Mr Bell thought that the point of interest in Dr Macdougall's paper was, as Professor Spence had said, that the removal of the bone with a tendency to death, had cut short a long and tedious process. He had lately had some cases of acute necrosis in his own wards. In two such cases, one, when admitted, was suffering from chronic pyæmia, the other from irritative fever. In the latter case, abscesses had been opened, and therefore no special treatment could be adopted. In both, the ulna was removed from end to end, and the result was complete osseous reproduction. In fact, the bone was thicker than before. In the ulna, however, better results could be got than in the tibia. He had been much interested in Dr Macdougall's paper.

Dr Macdougall, in reply, thanked the Society for their kind reception of his paper. All the anxiety given him by these cases was well repaid. In regard to Mr Chiene's remarks on acute necrosis, he could have removed, in the first case, the whole shaft, when he made his first incision. It was only the fear of shock, and the grave constitutional condition of the patient, that prevented him. In several such cases in the early stages, he had cut down without antiseptic precautions, and no exfoliation had followed.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIV.—MEETING IV.

Wednesday, 27th January.—Dr MATTHEWS DUNCAN, *President, in the Chair.*

I. Exhibition of a PLASTER CAST of a group illustrating the process of PARTURITION, found in the Temple of Venus, at Golgos, in Cyprus, by Mr Bibby of London. In his letter offering this cast to the Society, Mr Bibby pointed out that it consisted of four figures, namely, the mother, the nurse, the new-born infant, and the attendant. The group was chiefly interesting on account of the stool (mentioned in Exodus), and is supposed to be very rare, if not unique. The graceful and natural lines seen in the attitude and drapery of the mother suggest Greek art. He had presented a copy to the Obstetrical Society of London, and had much pleasure in furnishing medical men in Edinburgh and in Scotland, and Biblical archæologists, with an opportunity of seeing this relic of ancient times.

Dr Simpson proposed a vote of thanks to Mr Bibby for his welcome present, and instructed the Secretary to convey the thanks of the Society to him. He mentioned that there were at present two midwifery chairs in the Midwifery Museum belonging to the University, one of which had formerly belonged to the Royal Infirmary, and the other had been until quite recently in use in Malta. Kilian had given quite a long list of the various chairs and beds which had been used in parturition in various countries. In Holland, every bride when married used to take with her a confinement chair as part of her trousseau.

Dr Gordon remarked on the fact that in the cast the obstetrician appeared to stand or sit in front of the patient, and observed that this coincided with the etymology of the word *obstetricus*.

II. FŒTAL MONSTROSITY (PHOCO-MELOS). BY DR MACDOUGALL OF GALASHIELS. (COMMUNICATED BY THE PRESIDENT.)

Early on the morning of the 12th September 1873, I was requested to visit H. J., an unmarried girl, who was said to have become suddenly ill. I found her relatives anxious and alarmed, and totally unsuspicious of her condition. The membranes having suddenly ruptured, the bedding on which she lay was soaked with amniotic fluid. External examination revealed enlargement of the uterus corresponding to a seventh-month's pregnancy, while the finger inside detected an os but slightly dilated, with a highly situated and somewhat obscure presentation. As yet she had no uterine pains.

Six hours after, I again saw her; the os had become somewhat softer and the presentation more distinct. Its softness and general contour gave the impression of a breech, which, however, external manipulation did not verify. As matters progressed and a more complete examination could be made, a small and peculiarly soft head could be diagnosed. Once clear of the os, the child was rapidly expelled. The moulding at the perineal orifice was a short process. Before withdrawing it from under the clothes, some of its peculiarities were evident, notably the shortening of the lower extremities. It cried feebly when born, and lived for some minutes. Not anencephalous, but having no bony development in the vault of the cranium, apparently wanting thighs and upper arms, the monstrosity can hardly be regarded as a common one. In this instance, as in all the anencephalous cases I have seen, the liquor amnii was in very great excess.

Dr Keiller considered the specimen now exhibited to be one of very great interest, and remarked that the extremities were particularly defective; several of the fingers were webbed. There was no sign of intrauterine amputation of any part, nor was there spina bifida, exomphalos, or any mesial deficiency such as occasionally occurred in fœtal malformations of the kind. In a somewhat similar specimen in his possession, there appeared to have been, previous to birth, a compound fracture of one leg, as there was found a scar in the skin overlying a healed fracture of the bone. At the request of the late Sir James Simpson he (Dr Keiller) collected numerous cases of intrauterine amputations and other malformations. He had drawings made of several of them. These cases and drawings had never as yet been published; he hoped to do so, or at all events to bring them up before the Society. One of the most singular of these cases had deformities of all the extremities. In one hand the fingers were webbed, the other arm was amputated above the elbow; both legs were amputated, one below, the other above the knee. Another was a lady who had lived to adult life, in whom an arm had been amputated *in utero*, and who now wore a false hand. Dr Keiller had fallen in with five such cases, and had given drawings and descriptions of them to the late Professor Simpson with a view to their publication in his own excellent writings on this interesting subject.

III. HAIR IN THE FEMALE BLADDER. BY DR MACDOUGALL OF GALASHIELS. (COMMUNICATED BY THE PRESIDENT.)

In April 1874, I saw an elderly lady of corpulent habit, who was complaining of symptoms of gravel. The history she gave was that, until within the last two weeks, *at no previous time* had she had any urinary ailments. The first symptom was frequency of micturition, attended by very considerable pain. This pain she referred to the lower part of the abdomen. It certainly, she stated, was *not* aggravated by emptying the bladder.

Of full habit of body, living freely, and having comparatively little exercise, I was not surprised at such a history as she gave, and prescribed some antilithic remedies. No relief followed, the pain became more intense, and the calls to urinate more frequent. Now, however, she stated that the pain was greatest near the vulva. Digital examination enabled me to detect, lodged in the urethra, and in part projecting from its lower orifice, the mass of hair now exhibited. It was easily seized and removed, and with its removal all irritation ceased.

How it arrived in the bladder is matter of difficult explanation. Apparently ovarian in its origin, its transposition is curious. She had never suffered pelvic pain or uneasiness, and had no recollection of passing at any time a more than ordinary amount of urine, such as sometimes occurs when an ovarian cyst opens into the bladder.

Dr Simpson regretted that the history of this case was not more fully given; as it was, there appeared to have been no history of ovarian distress. Cases of hair in ovarian cysts every one must have seen, but he did not see how the hair in this case could have got into the bladder in such a way.

Dr Young said that this opened up the subject he had brought up in a paper he had recently read, where he had mentioned hair as being not unfrequently found in the bladder; but he could not explain its presence there.

IV. FIBROID POLYPUS. BY DR MACDOUGALL OF GALASHIELS. (COMMUNICATED BY THE PRESIDENT.)

On the evening of the 16th of June last, I was hurriedly summoned to visit Mrs G., a married woman, aged 40, who was said to be very ill. I found her pale and exsanguine, with a cold clammy skin and feeble pulse, restless and anxious, exhibiting in a marked manner all the bad symptoms of hæmorrhage. I was informed that she had prolapse of the womb, that it had come down some hours before, and that the bleeding had been profuse ever since. Between her thighs lay a large pear-shaped tumour, with a soft velvety surface, bathed in blood. No os uteri was visible, and the finger introduced into the vagina could detect high up the cervix, through which the pedicle of the tumour passed. The diagnosis was simple, the treatment more debatable. My difficulty lay between the use of the ecraseur or the ligature. Ultimately I determined to tie it; and, having the tumour drawn well down, I passed a fine whip-cord ligature round the pedicle just within the os. The traction upon the tumour was still maintained, and, to my dismay, in tightening the ligature, it *divided the pedicle*. Instinctively I grasped the cervix, dreading severe hæmorrhage would occur. My alarm, however, was groundless. After waiting some little time, during which the cervix retracted almost beyond reach, I carefully plugged the vagina, leaving instructions that watch should be kept lest the bleeding recurred. Happily it did not, and the woman, though weak and debilitated, made an excellent recovery.

Examination by the uterine sound some days after gave only a length of four inches. The growth itself weighed *thirteen ounces*, and at the spot where the pedicle was divided, the open mouths of some large vessels showed how great its vascular supply had been.

One or two points in this case are of interest. She had been the subject of the tumour for some years; the constant reddish watery discharge, a discharge which at her menstrual periods became excessive, and the bearing-down sensation, almost her only troubles. Six months before I saw her, it had first become prolapsed, and a medical man who visited her, mistaking it for the uterus, had, with much trouble to himself and pain to the patient, reduced it. A pessary was then recommended, but its use I suspect was not persevered in. The second occasion upon which it appeared externally, I saw and removed it. The effect of the ligature was happier than one had expected. My primary intention was, after in this way securing the vessels, to have removed the tumour with the knife. The fault I think lay in the continued traction upon the pedicle. The ligature once in position, had the parts been relaxed, an accident, which in some ways might have proved disastrous, would have been less likely to happen.

Dr Young remarked that these polypi were not uncommon; he had seen many of them.

Dr Keiller thought that cases of pediculated intra-uterine or cervical fibroid polypi were among the most satisfactory that we meet with as regards operative results. A good deal of the success of our special treatment depends upon the time at which they come under our notice; if we get them when they have escaped from the os uteri, and are ready to be removed, they are not only most satisfactory to treat, but, from the almost immediate cessation of the, it may have been long-continued, symptoms, prove unusually gratifying to the patient and friends. Years ago, he was in the habit of using the ecraseur for their removal, but now he cuts them off at once by means of the knife or scissors, and is seldom troubled with much bleeding, either during or subsequent to the operation.

Dr Simpson remarked that the division of the pedicle of intra-uterine polypi by the knife was usually followed by little or no bleeding; the pedicle being drawn back, the uterus contracts and the bleeding stops; but the case was very different with polypi growing from the cervix; he had once cut across a small mucous one growing from the cervix in which the hæmorrhage was very difficult to restrain, requiring the use of strong styptics and plugging the vagina and uterus. If there were any risk of this occurring, the galvanic cautery, which he frequently used in gynæcological operations, was the best instrument to use.

Dr Keiller confirmed Dr Simpson's statement, that mucous polypi were sometimes very treacherous, so were even small fibrous polypi growing from the cervix. In one case he delayed operating and introduced a sponge-tent, in order to open up the os uteri more freely. The patient, who had long suffered from hæmorrhage, died before he had been able to operate. He regretted having recourse to the sponge-tent, and was sorry, especially after seeing the size and trifling attachment of the polypus, that he had not at once removed it by means of the knife or scissors. When he (Dr Keiller) exhibited the preparation of the uterus with its still attached polypus to the Society, Sir J. Simpson called attention to the fatality which occasionally attends apparently unimportant operations, and stated that they were, of all our cases, not only the most painful, but often the most inexplicable.

V. CASE OF CENTRAL RUPTURE OF THE PERINEUM. BY DR WILSON OF ALLOA. (COMMUNICATED BY THE PRESIDENT.)

I was asked, about 10 A.M. on the forenoon of Friday the 3d April 1874, to attend Mrs N., æt. 26, in her first confinement. On visiting her shortly afterwards, I learned that she had had more or less severe pains in the abdomen since 3 A.M., and, on examination per vaginam, found the os uteri hardly admitted the point of the finger, while the vaginal surface was free from secretion.. I ordered her a dose of castor-oil, and asked the person in attendance to send for me when she thought it necessary. I was sent for about 4 P.M., and on my arrival found the pains very constant and severe, the membranes ruptured, and the head pressing strongly against the perineum, with a rigid vulva. At once I supported the perineum with my hand, and for nearly an hour continued to do so, the vulva in the interval becoming greatly relaxed. The head was now so far protruded, that I expected its exit each pain, but suddenly the perineum became thinned, gave way, and the head, receding from the vulva, escaped through the laceration, followed immediately by the body. I separated the child, passed the fingers through the vulva, and in a short time removed the placenta by the natural way. On examining the parts, the fourchette and sphincter ani were found entire.

In the evening, I wrote Dr Matthews Duncan asking his advice as to what should be done; but after thinking over the case, I resolved to bring the parts into apposition by means of the quilled suture, and, with the assistance of my friend Dr Duncanson, did so next morning.

The ordinary precautions in such cases were taken, and everything seemed to be going on well, till the Tuesday morning, when, to my great disappointment, the wound was found to be partly gaping; the patient, having been restless during the night, caused the looped end of the wires to slip from off the quill.

The surface of the lacerated wound being in a sloughy state, I ordered poultices containing carbolic acid to be applied for a few days; then stuffed the wound daily with lint, soaked in carbolic oil, and in three weeks after the day of her confinement the wound was entirely healed, the perineum being very slightly puckered.

Dr Burns had seen cases where the central part of the perineum was torn, the fourchette remaining uninjured, but never one in which the head had come through the rent.

Dr Hammond remarked, that Dr Polson of Friockheim, in Forfarshire, had informed him that a case had occurred in his practice in which central rupture of the perineum had taken place, and the child and placenta had come away through the rent; the mother recovered completely without treatment.

Dr Keiller had frequently seen cases similar to those referred to by Dr Burns, but never one like that related in this paper, in which the central laceration and expulsion were complete. He knew of such cases, and had seen preparations of the kind. Severe lacerations of the perineum were most common in cases of contracted outlet, where the pubic angle was small and the head consequently pushed far down against the perineal structures, before the movement of extension of the head took place. He was of opinion that in most cases the tear began in the centre, and that when the head came to be pushed out through the vulva, the perineum gave way from behind forwards; he had repeatedly examined and proved this in cases. The great principle to guide us in the management of these cases was to *save the central point* of the perineum, where the various perineal muscles meet; if it be torn through, these muscles pull the parts more asunder, and the wound consequently, being difficult to keep in apposition, gapes. Hence it was sometimes better, where such a laceration was to be feared, even to make incisions at the sides of the perineum, and so save the central or middle tendinous point. Where the accident has taken place, he placed most reliance on a few strong and deep sutures in the back part of the wound. It was unsatisfactory to put in too many, as the front part generally sloughed, and the stitches gave way. He referred to a preparation in Professor Simpson's museum, where a fistulous opening was left after this accident to the perineum. In this, as in Dr Wilson's case, the child had escaped bodily through the extensive central laceration, leaving the vaginal and anal borders of the perineum untorn.

Dr Simpson said that the case referred to occurred at Bathgate, and he had got the preparation on the death of the patient, a long time afterwards. He had seen the perineum torn in this way in a syphilitic case. He thought the sloughing in Dr Wilson's case was due to the quilled stiches being drawn too tight: he had seen this happen in an early case of his own. He could corroborate Dr Keiller's remarks about the rending most commonly beginning behind and extending forwards. The tear was generally central, but he had a case under treatment now where it was more to one side. He agreed with Dr Keiller that it was better, in cases of very tight perineum, to take the matter into our own hands, and make incisions on either side.

Dr Young said the only case of central rupture he had seen occurred in a primipara of about 40. Dr Moir, who saw the case with him, advised non-interference, and the patient recovered without any inconvenience.

Dr Gordon said that most cases of ruptured perineum recover without any treatment at all; he would like to have some guide as to which cases he ought to treat, and which to leave alone.

Dr Keiller remarked that in regard to treatment the guiding point should be the condition of the central or mesial line of the perineum; if this were torn through, sutures were immediately necessary. When he had wards in the Infirmary, he used frequently to perform the operation of renewing the perineum in cases of prolapsus, etc., many of which had resulted from lacerations of the perineum which had been neglected at the time of their occurrence. The ill effects often did not come on for years, and might have been avoided altogether by timely care and sutures. His experience of perineal laceration cases, and of the operations required for their radical cure, had been extensive, which led him still firmly to believe in what he on former occasions had stated in regard to this too much overlooked matter.

SESSION XXXIV.—MEETING V.

Wednesday, 10th Feb.—Dr J. MATTHEWS DUNCAN, *President, in the Chair.*

The President said that, before entering upon the public business, he should like to refer to a loss the Society had sustained in the death of an ex-President, Dr Thomson of Dalkeith. He would not offer any panegyric on the deceased, for he felt assured that it was unnecessary; suffice it to say that, as was well known to the Fellows, Dr Thomson had, more particularly during the tenure of his office as President, shown a warm interest in the affairs and prosperity of the Society, and endeavoured to the utmost of his powers to promote its success.

Dr R. P. Ritchie referred also in feeling terms to the demise of Dr Thomson, and proposed that the Secretary should be instructed to communicate to his representatives the unanimous sense of the Fellows as to the loss sustained, and their warm sympathy with the family under their bereavement.

Professor Simpson seconded the motion, and briefly referred to the interest Dr Thomson had taken in the welfare of the Obstetrical Society.

I. *Dr Affleck* showed the LIVER of a CHILD 17 months old affected with cancer. The case had been under his observation four months, and the nature of the disease had been recognised from the first. The post-mortem examination was made just before the meeting. The liver was found to fill the cavity of the abdomen, but the disease was confined to the right lobe, which was one huge mass of soft cancer, the greater part of the liver substance having disappeared. There were cancerous deposits in the mesenteric glands and also in both lungs. All the other viscera were free from the disease. The child had been perfectly healthy till the age of one year, and the family history was remarkably good.

The President said that the Society was certainly much indebted to Dr Affleck for exhibiting this preparation. Cancer in one so young, as Dr Affleck had remarked, was undoubtedly very rare.

II. CONTRIBUTIONS TO THE DETERMINATION OF THE DIMINUTION OF THE UTERUS AFTER DELIVERY. BY AR. SERDUKOF, M.D., MOSCOW. (COMMUNICATED BY THE PRESIDENT.)

(This paper will appear in an early Number of the Journal.)

The President said he had much pleasure in reading this elaborate paper to the Society. It was a careful and thoroughly scientific essay. Dr Serdukof had not indulged in theory: the basis of his conclusions was careful measurement. It was most interesting and satisfactory to note that the measurements of Serdukof—new, so far as he was aware—had not conducted to new views, but had confirmed notions previously widely held. This did not depreciate his work, for it was quite necessary to have these beliefs confirmed.

He referred particularly to the proof that had been afforded them of more tardy involution in premature cases; of more satisfactory, steady, and speedy involution, after the first week, in primiparæ than multiparæ, in healthy women and in easy labours. The paper had deeply important practical bearings; for instance, on endeavouring to get women on to the full time, in the management of nursing, and securing, if possible, exemption from such evils as parametritis, etc.

Professor Simpson agreed very much with Dr Duncan, only he should like to point out that Dr Serdukof may not have been the first to undertake these post-partum measurements. In point of fact, Dr Bermer, an Austrian physician, had made very careful measurements, not only between the fundus and symphysis, but on the anterior part of the uterus. For internal measurements he had employed a thicker rod than the sound, thus obviating the defects to which the latter was liable here.

Dr Macdonald doubted whether more than relative correctness of measurement could be arrived at by the method of Serdukof. It would not be easy to arrive at absolute correctness, owing, among other things, to the varying thickness of the abdominal wall. As regarded the question of priority, it struck him that an Edinburgh graduate had, about the year 1864, measured the uterus after delivery, but his plan was internal.

III. *Dr James Young's* "CASE OF SUPPOSED NEPHRO-CYSTOSIS DURING PREGNANCY," with operation. The discussion on this paper was delayed until Dr Young was able to report the subsequent history of the case.

NUNEATON AND COVENTRY.

IT is the general tenor of our occupation to record progress and to accord praise; but it is only just, and it is undoubtedly wholesome, to consider occasionally our failings, our disgrace, and even our dishonour.

The 1875 assizes of Nuneaton and Coventry are and will be unpleasantly memorable in the annals of our profession. In each of these places, trials of medical practitioners have taken place and have been recorded since our last issue, when we took occasion to animadvert on the foolish conduct in which Inspector Elms took an inferior but striking part.

At both assizes, the charge was manslaughter. In the Nuneaton case, the criminal was a medical man who cut off a large portion of the intestines of a living woman, expelled during labour, and made attempt to conceal what he had done. He was defended by a London obstetrician and others of great eminence, whose evidence was most severely characterized both by the bar and the bench. The prisoner was brought in guilty, and had as light a sentence as might be recorded against him.

In the Coventry case, the prisoner was a midwife. She was charged with causing the deaths of some of her patients, by infecting them with puerperal fever—a charge, we believe, never before heard of in the annals of the law. With the approval of the presiding judge, she was brought in not guilty.

There cannot be a doubt that in both cases justice has been done. A medical man is punished for criminal mismanagement of a midwifery case; the character of his defence is impugned; a midwife is acquitted.

The conduct of the medical press and of the *Times* regarding the latter of these cases, is well worthy of a little attention. At present there is, we believe, in the profession, and in that part of the public which takes any interest in the matter, a strong desire to have the midwives of the country improved in education, and legally recognised as practitioners. But in choosing the present time and its events for the purpose of advancing these excellent measures, as well as in their criticisms of these events, journalistic editors have been most inaccurate, unfair, and unfortunate.

The burden of these articles is the ignorance of midwives and the mischief they do. The lesson of the events is, the misconduct of a regular practitioner, the absurd statements of his defenders, and the mischief they do, and are punished or reproved for doing. Ignorance may be assumed in the midwife, but there is not even an attempt to prove it.

The midwife was not proved to be careless or foolish in her conduct. She may have been so. We have no code, written or unwritten but generally approved, by which to judge her conduct. While every one must admit that there is much and widely-felt need for some understanding as to the proper conduct of physicians, surgeons, and obstetricians, when they are in attendance on infectious diseases, it is plain that, in the absence of such, it was cruel and improper to bring a midwife under a criminal charge; and we cannot but disapprove the conduct of all who have aided or abetted the proceeding, and this despite the excellence of their motives.

The midwife in the present case was, so far as we know, merely doing what is generally done by physicians, surgeons, and obstetricians alike; and it is very curious to notice that, in this very case, one of the witnesses, evidently without any consciousness of impropriety in his conduct, accidentally described himself as doing what the prisoner at the bar had done. Besides, we have recently had article on article concerning puerperal fever, where the ingenuous practitioners describe themselves as doing what the midwife did, and we do not find their conduct animadverted upon as criminal. Certainly, none of them has been treated in the disgraceful manner in which the midwife has been used.

We have been much interested in the interference of the great *Times* in this medical matter, and have been greatly disappointed.

Had the editor of this paper taken merely a popular view of the subject, we should have left him to wallow in his errors. But the article is evidently inspired, if not written, by a medical man, and aims at a scientific character. We must, therefore, quote a passage from it in order to mark it with disapproval. The passage is as follows, and we need scarcely tell our readers that it is in about an equal degree not sense and not accurate, wrong in its theoretical notions, and in its statements of alleged facts:—

"It is the invariable practice of medical men, if they attend a case of this fever, to hand over the whole of their midwifery practice to other persons for at least two or three months; and it has been shown, by ample experience, that this course is absolutely necessary to preserve the lives of their patients." (2d March.)

Such are the absurd statements of an acknowledged leader of public opinion.

Part Fourth.

PERISCOPE.

MM. **Budin and Coyne** have noted the state of the pupil in different stages of anæsthesia from chloroform in patients undergoing surgical operations, and in experiments on dogs. From the observations and experiments they draw the following conclusions:—

1. The administration of chloroform produces a series of modifications in the state of the pupil which have a relation to the state of consciousness.
2. During the period of excitement, when it is defined, the pupil is dilated.
3. This period passed, the pupil contracts progressively, although still capable of reacting.
4. During the period of deep surgical anæsthesia, two constant phenomena are observed as regards the pupil:—(*a*) An absolute immobility of this organ; (*b*) A state of contraction. There is a relation between the absolute insensibility of the patient and the contraction with immobility of the pupil, between the return to sensibility and the dilatation with mobility of the organ.
5. The state of the pupil can thus, from the point of view of its sensibility, serve as a guide in the administration of chloroform.
6. Slow dilatation of the pupil coming on during the operation indicates that the anæsthesia is less profound, and that the return of consciousness is near.
7. During operations of long duration, if it is desired that the patient remain completely insensible, the anæsthesia should be so managed that the pupils remain constantly contracted and immobile.

8. Efforts of vomiting can produce dilatation of the pupils, cause the insensibility to disappear, and cause the patient to awake; they annihilate, to a certain extent, the effects of the anæsthetic.

9. It is important, from the point of view of the phenomena connected with the state of the pupil, that the true anæsthesia from chloroform be not confounded with anæsthesia from asphyxia. The latter produces modifications in the state of the pupil of a different kind.

10. Although the state of the iris can serve as a guide in producing anæsthesia, it does not warn against danger. It is always the pulse, the respiration, and the general state of the patient, that the chloroformist should watch attentively.—*Archives de Physiologie*, Janvier-Février 1875.

H. CHOUPPE. *Researches into the Mode of Action of the Emetics most Commonly Used.*—The author sums up as follows:—From all the experiments which I have made, and from the discussion resulting from them, it seems that the following propositions are established. The mode of action of the most used emetics is not the same for all of them, great differences existing in the phenomena which accompany or precede the act of vomiting.

Ipecacuanha, and its alkaloid emetine, in whatever way they are introduced into the organism, always provoke vomiting by a direct irritation of the terminal fibres of the pneumogastrics in the mucous membrane of the stomach.

Tartar emetic and apomorphine have a double action: they can act upon the gastric mucous membrane, but also directly on the medulla oblongata. There are nevertheless differences between these two drugs; tartar emetic acts more rapidly on the stomach than on the medulla, apomorphine more rapidly on the nervous centres than on the gastric mucous membrane. The best proof of this that can be given is, that doses of tartar emetic sufficient to produce vomiting require to be stronger when this substance is injected into the veins than when it is introduced into the stomach. With apomorphine, on the contrary, it is by injection into the general circulation that the maximum effect is produced.—*Archives de Physiologie*, Janvier-Février 1875.

E. MENDEL. *The Temperature of the External Ear in Physiological and in Pathological Conditions.* (*Virch. Arch.* 1874, lxii. 132–141.)—The temperature of the external ear is in health constantly 0·1–0·3°, on an average 0·2° (Centigrade), lower than that of the rectum. During the apoplectic or epileptiform attacks in paralytics the temperature of the ear, compared with that of the rectum, was found to have risen. On the other hand, the temperature of the external ear sinks considerably in sleep produced by chloral and under the influence of subcutaneous injections of morphia, whence the conclusion is drawn that in poisoning by narcotics, at least by chloral and morphia, the application of bladders of ice on the head cannot be considered a rational therapeutic measure.—*From the Centralblatt*, No. 9, 1875.

G. L. POUZA. *La Transfusione del Sangue negli alienati.* Alessandria.—In three cases of insanity resulting from the disease of the skin termed pellagra, in which the physical strength was also much reduced, the author practised on repeated occasions transfusion of undefibrinated lamb's blood. The influence on the improvement of the bodily and mental condition of the patients was so striking that he recommends this operation in the strongest manner.—*From the Centralblatt*, No. 4, 1875.

FOURNIER ON SYPHILITIC GUMMATA (*concluded from p.* 656.)

COMPLICATIONS.—The principal complications of the gumma arise either through the evolution of the gummy ulcer, or the reaction exercised by the tumour on the surrounding integuments.

In some cases, the ulceration of the skin, following the evacuation of a gumma—especially when neglected—persists for a long time. The gummy ulcer may progress and extend itself, destroying the whole thickness of the integuments, and may even take on a phagedænic action. Gummata can also create complications in their vicinity.

Thus, gummata of the head, when overlooked, may determine ostitis and necrosis of the skull, with the possible dangers attendant on such lesions.

REVIVAL OF GUMMATA AFTER HEALING.—When a gumma has gone through all its phases, and cicatrized, ulceration may yet recommence in the cicatrix.

One of our patients, severely affected with syphilis, had, among other lesions, a gumma of the foot. This gumma went through its stages, and cicatrized. Then some time after, cicatrization being well established, the cicatrix opened at several points; new ulcers were produced, deep, with sharply-cut edges and sloughy floors; in a word, exactly resembling the ulcer which had cicatrized.

DIAGNOSIS.—As in every other syphilitic lesion, the diagnosis of the gummy tumour rests upon three kinds of considerations—knowledge of antecedents, consideration of concomitant symptoms, and the special characters of the lesion. At present, we have to investigate the special characters of the lesion. The characters specially distinctive of the gummy tumour are its pathology and its evolution. Its peculiar pathological feature consists in the presence of the gummy core, a white and filamentous substance like slough, to which we have above referred. The evolution is still more characteristic: *a tumour at first solid, softening slowly and forming an ulcer having a very distinctive aspect.*

Tumour solid, tumour softened, consecutive ulceration—this is the triple phase through which the gumma passes. Although one of these stages of evolution may not be in itself pathognomonic, their combination is, without doubt, characteristic of the gumma.

Add to these certain attributes of secondary importance; the rounded form of tumour; indolence at commencement; size in general, medium; central softening; nature of fluid evacuated; hollowed-out aspect of consecutive ulcer; sluggish progress of the ulcer; etc. It is in these divers symptoms that we find useful aids to diagnosis. In the differential diagnosis of syphilitic gummata, the separation of this affection from cancer is the most important.

Four kinds of considerations help the diagnosis:—1*st*, etiological considerations; 2*d*, differences in the symptoms proper to each lesion; 3*d*, state of the glands; 4*th*, influence of treatment.

1*st*, *Etiology.*—The gumma is always preceded by specific accidents, and may be accompanied by *actual specific* lesions. Nothing of the kind occurs in cancer—always reserving the possibility of cancer occurring in a person previously syphilitic.

Cancer more commonly becomes developed in persons of advanced age, and among those hereditarily predisposed. The gumma, on the contrary, has no special age of predilection.

2*d*, *Clinical characters of the lesion.*—

Cancer.	*Gumma.*
Tumour almost always single.	Sometimes single; often multiple.
Tumour of any size, and generally large.	Tumour small, or at most medium size.
Tumour irregular in form, not circumscribed, with indented surface.	Tumour regularly ovoid, rarely indented.
Tumour adherent, or rapidly becoming adherent, to the skin.	Tumour not adherent to the skin.
Tumour painful, lancinating pains.	Tumour indolent.

In the stage of ulceration there are also distinctive differences between cancer and the gumma. Thus, the cancerous ulcer makes its way from the surface to the centre, is irregular, fungoid, fetid, hæmorrhagic, large in extent, etc., and is very different from a gummy ulcer, which is the result of central softening, subsequent elimination of a slough, and never presents appearances resembling cancerous granulations, etc.

3*d*, *Condition of the Glands.*—

In Cancer.	*With the Gumma.*
Glands affected very rapidly.	Glands intact.

An excellent sign, which, with very few exceptions, suffices (for a certain period at least) to distinguish cancer from the gumma.

4*th*, *Treatment.*—In cancer, no action is obtained with the iodides or mercurials. With the gumma, these remedies have a rapid resolutive action, with rare exceptions.

PROGNOSIS.—Gummata of the cellular tissue are not in general grave affections directly. Suppuration more or less prolonged, destruction of the skin to a greater or less extent, and permanent cicatrices are what they lead to. But gummata are grave, chiefly on account of the condition of which their presence is the evidence.

They show that the syphilis has entered on that stage in which serious lesions may occur. From this point of view, however, there is a distinction to establish. An isolated gumma, two or three gummata even, have no more prognostic significance than any other tertiary lesion. They are evidence of the tertiary stage, and that is all. They do not announce a specially grave form of syphilis.

Multiple gummata, on the contrary, coming on simultaneously, or succeeding each other at short intervals, have an extremely grave significance. They are evidence of a severe form of syphilis, and the proof of this is, that multiple gummata figure habitually and in the first rank in the *cortége* of accidents which accompany the syphilitic cachexia.

Isolated gummata (or at least gummata that are not multiple) show a great tendeney to resolution under specific treatment. They are cured by iodide of potassium.

Multiple gummata, again, are much more rebellious, often removed at one place, only to appear somewhere else, sometimes resisting treatment altogether.

TREATMENT.—Before mentioning what onght to be done to combat the lesions we are considering, we commence by detailing what ought *not* to be done.

1st, Gummata ought not to be removed. It is a perfectly useless operation, when iodide of potassium can take the place of the bistoury. Gummata have also been observed to return shortly after ablation.

2d, The application of caustics is as useless for the same reasons as the preceding operation.

3d, Abstain from opening a gumma, even when fluctuation is present. And why? Because resolution of the gumma is possible in all its stages, even during the period of softening and when the tumour fluctuates. Examples abound where fluctuating gummata, on the point of opening, have been seen to diminish and become absorbed under the action of iodide of potassium.

These negative precepts having been set forth, let us see what the treatment to be adopted is. It is very simple; administer iodide of potassium at once in sufficient doses (one, two, three grammes), and in strong and rapidly increasing doses when the gumma is in an advanced stage of development. This resolves the gumma, and causes it to disappear with a rapidity truly astounding. It resolves gummata even of the largest size.

The iodide of potassium must, in the first instance, be always prescribed by itself; this in general suffices for the cure. It is rarely necessary to combine it with mercury. The indication calling for the administration of mercury along with the iodide, only presents itself in two orders of cases:—*1st,* Against precocious gummata when the syphilis is in an early stage, the time when mercury is most active; *2d,* Against gummata rebellious to other treatment.

In such cases it is also necessary, along with specifics, to administer tonics, for the production of gummata is frequently combined with a low state of the organism, either anterior and foreign to the diathesis, or engendered by it.

Lastly, A word on the local treatment of gummata.

During the first and second periods, treatment plays a very secondary part. There have been proposed, and often employed at this stage, certain local means as adjuvants to treatment, mercurial frictions, painting with tincture of iodine, blistering, etc. It is difficult to speak with certainty of the amount of benefit derived from these applications, as they are never employed alone. When the gumma opens, however, local treatment becomes most necessary. Whenever the gumma is on the road to elimination, some hygienic care is indispensable, as bathing and emollient cataplasms to moderate . the peripheral inflammation, detergent lotions injected into the gummy cavity. Lastly, when the gummy ulcer is formed, dressings are required, the most useful being the plaster de Vigo.

With the cure of the gumma the task of the physician is not completed; it is still necessary to watch the patient, and to continue the treatment of the diathesis long after the local affection is cured.

IODIDE OF POTASSIUM IN ANEURISM.—In the *American Journal of the Medical Sciences* for January last, Thomas M. Mathews, M.D., of Mount Enterprise, Rush Co., Texas, says—"During the past winter I was called to attend Mrs W., æt. 38, married some twelve or fifteen years, but had never had any children. I failed at the time to detect any cause for her general ill health, but in April last I discovered a large aneurism of the abdominal aorta, which on close examination I found to extend from under the sternum obliquely downward to below the umbilicus. Having been taught that such a condition was necessarily fatal, I should have given the case up as such had I not seen the article, on page 539 of the *Amer. Journ. of the Med. Sciences* for April 1874, by Dr George W. Balfour, on large doses of the iodide of potassium in such cases. As a *dernier ressort* I determined to give the drug a trial. I at once put the lady upon twelve and a half grains dissolved in the syrup of sarsaparilla three times a day, and increased the dose each day till it reached fifteen grains. I then gave fifteen grains four times in the twenty-four hours. *My patient began at once to improve;* the large doses seemed at first to irritate the stomach a little, but this soon passed away. When I began the use of the remedy the lady was confined closely to her bed; could not sleep unless under the influence of chloral; now, five months have passed, she is able to be up nearly all the while; sleeps without the soporific; rides about, even on horseback, a good deal, and is apparently in very good health. The aneurismal thrill and the bruit once so distinct are nearly absent, though the remains of the "sac" can be distinctly felt, yet very much smaller than it was at first. I used nothing but the iodide of potassium, except during June, when I gave between the iodide doses of fl. ext. ergot, ʒj., and tr. digitalis, gtt. v. I have kept up the use of the iodide steadily till now, except for two weeks; at present I am giving iodide of potassium, gr. v., and carbonate of ammonia, gr. iij., three times a day. Believing that in the potassic salt we have a remedy which will in a very great measure control, if not cure, this disease, which was formerly believed to be incurable, and hoping that others may be induced to try the remedy, I submit it to the consideration of the profession."

In the *American Practitioner*, published at Louisville, for November 1874, there is a paper on the Medical Study of Inebriety by T. D. Crothers, M.D. The following is an extract:—

In considering the pathology of inebriety, it is a pleasure to note that American authorities are far in advance of their transatlantic brethren on this subject. Dr Joseph Parrish, in a recent lecture,[1] says, "I believe the diseased condition which leads to intemperance to be one of alimentation chiefly. Our appetites differ widely. In some forms of disease there is eccentric diversity, as in hysteria and chlorosis. Here the articles eaten do not satisfy gustatory sense, but a constitutional demand. Such cases show a disordered alimentation and of the nerve functions which control it. . . . This is attributed to some unusual molecular change or metamorphosis in nerve-structure or some variation in the process of digestion."

Dr Willard Parker says,[2] "Our appetites simply express the wants of the system; but when abnormal or diseased cravings for alcohol, opium, or hasheesh exist, it indicates a diseased nutritive function demanding relief beyond the power of the will."

Dr George Burr concludes a valuable paper[3] on this subject as follows:—"1. Inebriety is a disease of constitutional character, involving the entire organism in its consequences; 2. That the true disease is a morbid craving for alcohol, of which the act of drinking is but the effect; 3. Whatever the true physiological action of alimentation may be, or in whichever system of nerves the sensation which gives rise to hunger and thirst may reside, the symptoms of inebriety indicate that the disease is a disordered condition of this function, and that the ordinary sensations of appetite for food and drink are perverted and uncontrollable." In an article read before the Albany County Medical Society, the author refers to this subject as follows:—"There are some reasons for believing that the brain is divided into certain districts, which preside over and take cognisance of organic life. The processes of nutrition are so intricate and varied, it is not unreasonable to suppose that certain brain districts preside over and co-ordinate its functions, determining the quantity and quality of food needed. When this function becomes deranged, unnatural, inordinate manifestations follow, more food than is required is taken, the quantity and quality ignored, perversions and aversions begin, and molecular changes follow beyond the power of analysis to determine. To summarize: 1. Inebriety is a disease of certain parts of the brain and of the nutritive functions under its control; 2. It is provoked by alcohol in variable quantities, depending upon unknown conditions of the body at the time of exposure; 3. A weakened will-power and low tone of physical vigour, with tendency to inebriety not inherited, are manifestations of the disturbance of the co-ordinating power of this function."

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[1] Before the Baltimore Medical and Chirurgical Society.

[2] See article in vol. iii. of American Association for Drunkards.

[3] See paper in vol. iii. of American Association for Cure of Inebriates.

Dr Parrish remarks,[1] "There are morbific causes, which lie concealed in the human constitution, which tend to the development ot various forms of disease. I think it safe to assume that in the germinal cell this element resides, which is certain to progress with the evolution of its forces and structure, as the cell itself is certain to develop into a human being; and that when the being is completed it will possess all the characteristics which resided primarily in the cell, including its physiological as well as its pathological tendencies; and also that each molecule, as it is dissipated in the process of disintegration and growth, leaves its impression upon its successor; and be that impression physiological or the reverse, it has the power of continuity until such continuity is interrupted and modified by the nutritive forces. Another fact equally important must be borne in mind, namely, that the life of a cell may terminate and give place to a successor of a new character, and thus the condition of the individual constitution assumes a new plane of life."

Of the treatment of inebriety, Dr Comings says, "The only successful treatment of this disease consists in the employment of such physical and moral means as shall enable the inebriate to persist in total abstinence, and so long as he can maintain this position he is said to be cured."

Physically, the inebriate has deranged nutritive functions, and from that a general debility of all the organs, with perverted will-power and weakened mental force. The function of repair by which the body is built up and sustained is interfered with. To reach complex disease, all efforts must be directed to building up the physical, and through that reach the moral and mental. Restore the inebriate to good health, then you can bring a strong mental co-operation in aid to complete the treatment.

The treatment may further be classified under two forms, reparative and restorative. The former removes all exciting causes and influences which increase the disease, thus lessening its intensity, and enabling the patient to partially recover and resume business again. A change of climate, surroundings, and association, with temporary medical help, will frequently do this; the present treatment of inebriety aims to accomplish this result generally.

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[1] See lecture before Baltimore Medical and Chirurgical Society, April 1874.

The periodic drinkers are the least hopeful. They are subject to paroxysms not unlike malarious poisoning, and during the frenzy seem bound by no restraint. After the attack, they make frequent and desperate efforts to save themselves, but fall as before. For this class the asylum offers the only possibility for recovery or stay of the disease, by building up the system and restraining the outburst until the victims are over the attack. It is asserted that there are over one hundred thousand of this class in this country, who are growing worse every day. Under proper medical treatment at an asylum, many of them would be enabled to regain their place in society and business.

By the restorative treatment, the patient recovers his health, and is cured in the same sense that a man with pleurisy or pneumonia becomes well again. He may be attacked again by wanton exposure to the causes. To accomplish either of these plans, there must be special surroundings and conveniences such as can be found in but few private families.

The general treatment may be summed up as follows:—1. Hospitals and asylums, built for the special purpose of treating inebriates, remove the exciting cause—viz., alcohol—and this is of the first importance. 2. They enable the patient to isolate himself from all excitement, and allow complete rest of the nervous system. 3. They remove all care and responsibility from the patient's mind, except that of recovering his lost health, and make him a party with the physician to bring about this result. This of itself is a powerful factor in the treatment. 4. Hospitals and asylums give the medical man complete control over the habits and surroundings of the patient, and offer facilities for superior hygienic and medical treatment.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario College of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

Action of Cantharides.—Dr Cantieré draws the following conclusions from his experiments:—Cantharides causes the blood-corpuscles to become crenated; it lessens the blood-pressure by diminishing the contractile power of the heart and arteries; it quickens the pulse and raises the temperature; it causes hyperæmia and stasis, and, if the dose be large, true inflammation in various organs. It causes softening of the cerebellum and of the cord chiefly at the lumbar enlargement. At the base of the brain and medulla oblongata hyperæmia is well marked. It induces also hyperæmia or inflammation of the urinary and genital system. It acts as an aphrodisiac. Internally administered, it produces ulceration of the gastro-intestinal canal; when injected into the blood, if it does not produce ulceration, it will cause diarrhœa.—*La Sperimentale*, tomo 34, fasc. 9 and 10.

TREATMENT OF RHEUMATIC FEVER BY CYNARA.—In 1832 Dr E. Copeman, the present President of the British Medical Association, drew the attention of the profession to this subject (*Medical Gazette*). He has since then employed the extract and tincture of cynara, the common artichoke, for the treatment of rheumatism with more satisfactory results than afforded by any other means. In writing on the subject in the *British Medical Journal*, December 1874, he says, "No other medicine has in my experience appeared to be so efficacious as cynara in rheumatism acute or chronic, but I have frequently heard of its having failed in other hands, and I suspect the reason is that it is often prepared from leaves gathered at an improper time when there is no medicinal virtue in the plant. The leaves should be gathered just before the top of the vegetable is fit for food and whilst they are full of juice; if left until the top is cut off for cooking purposes and the plant begins to wither, no good will be derived from it, and, if given, it would be condemned as useless and inert."

THE COMBINED ACTION OF ERGOT AND BORAX.—Borax is by some practitioners accredited with emmenagogue properties. Dr C. Brown, in cases where ergot is indicated, combines with a full dose of that drug equal proportions of borax, and finds that this combination is better borne and is more durable in its action than ergot alone.—*Trans. Med. Soc. Pennsylvania.*

PREPARATION OF ERGOTIN.—A preparation of ergotin suitable for subcutaneous injection has long been a desideratum, those in ordinary use being liable to cause pain and to produce abscesses. In a recent communication to the Berlin Medical Society, Wernich recommended the following method of preparing ergotin for hypodermic injection:—A watery extract is prepared from powdered ergot, which had been previously deprived of all fatty matter by ether, and then treated with alcohol. The watery extract is next purified by diffusion through parchment paper, and will form a clear brown solution in water.—*Allgemeine Medicin Centralzeitung*, Nos. 35 and 36.

JABORANDI.—M. Albert Robin recently communicated to the Société de Thérapeutique the result of his investigations as to the effect of this drug (*Gaz. Hebd.*, 24th Nov. 1874). He found that the urea and urine are at first diminished in quantity, but that after some days the quantity of these and of uric acid and chlorides is increased. Jaborandi seems to act on the vaso-motors, diminishing tension by paralyzing them and facilitating the passage of blood through the cutaneous capillaries. In cases in which it was given in minutely divided doses there was no sweating or salivation, but a copious diuresis.

MM. Ball and Hardy lately stated to the Société de Biologie that they have ascertained by experiment that the quantity of the urea excreted in the urine is always diminished under the influence of jaborandi, and that the sweat always contains a portion of urea; but on adding these two quantities, the total was (except in one case) always inferior to the amount excreted in the urine the day before the administration of the drug (*Gazette Med.*, 21st Nov. 1874).

Mr W. Martindale, F.C.S., has an interesting paper on jaborandi in the *Pharmaceutical Journal*, 7th Nov. 1874. Dr S. Coutinho, he states, brought to Paris, from Pernambuco, a plant called "Jaborandi" by the Indians. This is a shrub which grows in the interior of some provinces of the north of Brazil. We cannot yet say with certainty the plant and order yielding jaborandi. Prof. Baillon believes it to be identical with Pilocarpus pinnatifolius, N. O. Rutaceæ, and Prof. Oliver also believes it to be a rutaceous plant. Mr Martindale seems to think it possible that it may be the produce of one of the Piperaceæ, and certainly there is a species of pepper called jaborandi—the Piper jaborandi. With regard to the chemistry of this plant, M. Rabuteau finds that it contains an inert volatile principle, an inert substance, tasteless and soluble in water, and a substance with a bitter taste and soluble in alcohol, on which the effects of the drug depend. He did not find any alkaloid or definite active principle. The effects of jaborandi have been tried in this country by Prof. Sydney Ringer (*Practitioner*, December). In three out of four cases he found it to cause copious perspiration. In one case the skin remained quite dry. It acts as a sialagogue, sometimes causing a very great flow of salivary secretion; but its sialagogic action is less uniform than its diaphoretic action. In one case only did it increase the bronchial secretion. It accelerates the action of the heart, and renders the arterial impulse more visible. In each observation the temperature fell considerably. In all cases vomiting was excited, but with scarcely any nausea. It produced drowsiness, which supervened when the flush disappeared from the face and the skin became pale. The perspiration continued profuse long after the flush had left the face.

The dose of jaborandi is from 30 to 90 grs. of the powdered leaves, infused in boiling water, and taken without straining. M. Gubler states that it sometimes produces diarrhœa.

A leader in the *British Medical Journal* for 27th February gives an excellent summary of the facts concerning jaborandi which have been hitherto ascertained. Sphygmographic tracings show that jaborandi causes marked lowering of the systemic blood-pressure. Children are less susceptible to its action than adults, as noted by Ringer and Gould. In this particular, and in quickening the pulse and flushing the face, it resembles belladonna; but it exerts an opposite action to that drug in the salivary, sudoral, and mammary secretions, on the pupil, and on the minute arteries. Belladonna also tends to cause delirium, while jaborandi causes prostration and sleepiness. The influence of atropia on the salivary flow is due, according to Heidenhain's experiments, to paralysis of the terminations of the secretory filaments of the chorda tympani. Muscarin, as shown by Schmiedeberg and Koppe, is antagonistic to atropia in this respect, causing salivation by stimulating the secretory filaments of the chorda tympani, and jaborandi appears to act similarly. Tweedy has shown that the topical application of an extract of jaborandi causes contraction of the pupil and temporary myopia, due to spasm of accommodation. With regard to the therapeutical application of jaborandi, the writer suggests that it may do good service at the outset of acute febrile disorders, such as simple pneumonia, when the skin is hot and pungent. It may also be useful in some forms of Bright's disease, when the arterial tension becomes high, and the skin cannot be made to act by hot-air baths. The depressant action of jaborandi on the heart should be remembered; it has been known to do harm in rheumatic fever, complicated by endocarditis. It may find a place in ophthalmic practice, and may prove an antidote to poisoning by belladonna. At a recent meeting of the Société de Thérapeutique, Professor Gubler brought forward some additional remarks concerning jaborandi. A large parcel of the leaves and wood has lately arrived in France. Gubler finds that 6 grammes of the bark are equal in power to 4 grammes of the leaves; he finds, also, that the black-coloured leaves are less active than the green. The drug sometimes produces vomiting, and the diaphoresis is sometimes replaced by diuresis. The sialagogue effects are sometimes so considerable as to cause great prostration. Jaborandi is indicated in anasarca and œdema met with in rheumatism, albuminuria, and diseases of the heart. In chronic bronchitis, and in emphysema with asthmatic paroxysms, jaborandi has yielded in Gubler's hands remarkably good results. He has also given it advantageously in subacute rheumatism.

MONOBROMIDE OF CAMPHOR.—In a former "Report" we alluded to Bourneville's experiments with this substance.

Monobromide of camphor was discovered by Swarts in 1862. It is a definite substance, in which an equivalent of the hydrogen of the camphor is replaced by one of bromine. In a paper by M. Gault, in *L'Union Pharmaceutique*, vol. xv. p. 266, the mode of preparing it is given in detail. It consists of hard crystalline acicular colourless prisms, with an odour of camphor and turpentine, and with a slightly bitter camphor-like taste. It is insoluble in water, but soluble in alcohol, fixed and volatile oils, ether, and chloroform. Dr Lawson, in a paper in the *Practitioner* for November, calls attention to the difficulty of finding a suitable form for administering this remedy. The elegant dragées of Dr Clin form probably the best method of administration. Each dragée contains 10 centigrammes (1½ grains), covered with a thin coat of sugar, which preserves the drug, masks its odour and flavour, and renders it easy of deglutition. The dose is not yet precisely ascertained; probably it would be from twenty centigrammes to a gramme, according to circumstances.

Part Fifth.

MEDICAL NEWS.

Presentation to Dr Haldane.—We understand that, on Friday the 5th of March, an extraordinary meeting of the Royal College of Physicians was held for the purpose of presenting to Dr Haldane, the late Honorary Secretary, a testimonial which might worthily express the sense, unanimously entertained by the Fellows, of his great and valuable services to the College during his long tenure of office. The testimonial took the form of a handsome service of silver plate, value three hundred guineas, partly, we believe, made of silver from Scottish mines. It bore an inscription, which, in appropriate terms, referred to Dr Haldane's long and valuable services, and testified to the high esteem in which he is held by the Fellows of the College. The meeting, we understand, was a very successful one, and Dr Haldane was warmly congratulated by all the Fellows present. Dr Haldane, we believe, held the office of Secretary to the College for eighteen years, being elected in November 1856, and having resigned in November 1874. The very able and energetic manner in which he fulfilled the duties of the office is well known to the profession, who will feel satisfied that a better helmsman could not have been found, during a period of transition and unrest in all the colleges and corporations; and this proof of the gratitude of his College will give much pleasure to his many friends.

Royal Society—Makdougall Brisbane Prize.—The Council having awarded the Makdougall Brisbane Prize for the Biennial Period, 1872–74, to Professor Lister, for his paper "On the Germ Theory of Putrefaction and other Fermentative Changes," the medal was presented to him by the Chairman on Monday, 15th March 1875.

The Council have awarded the Neil Prize for the Triennial Period, 1871–74, to Mr Charles William Peach, for his contributions to Scottish Zoology and Geology, and for his recent contributions to Fossil Botany.

AMENITIES OF PRACTICE IN CHILI.—The following has been added to the Chilian Penal Code:—"The medical man who, observing in a person or in a dead body signs of poisoning or other grave crime, does not give immediate information to the authorities, will suffer the punishment of from 21 to 60 days' imprisonment or a fine not exceeding 100 dollars."—*Revista Medica de Chile*, December 1874.

From the same source we learn that the Medical Society of Santiago has resolved to give an annual prize for the best two essays on a given subject. The theme announced for the present year is "the ascertained use of the varieties of climate which Chili offers for the cure of diseases and the restoration of convalescents." The essays have to be given in not later than 1st July.

PROFESSOR GAIRDNER ON EXAMINATIONS.—The closing address at the Glasgow University Medical Society was delivered by Professor Gairdner in the Physiological Class-room of the University. After some preliminary remarks upon the Society and its objects, Professor Gairdner said he intended to speak upon "Examinations: their relation to teaching, and their proper object, especially the object of pass examinations." He referred to the work of Mr Todhunter, and dissented from his opinion that the natural sciences were less fitted for training and disciplining the mind than the older mathematical and logical sciences, and he found in the great extent and variety of the natural sciences simply a reason for altering the methods both of teaching and of examination in these rather than of keeping them out of the curriculum of the student. Of course it must be admitted that the primary object of examination is to eliminate incompetent candidates. But when it is considered that for every man rejected there are many that are passed, the influence of the examination upon the studies of those that pass will be seen to be much more important than its function as a mere measure of police. The great extent of almost all the natural sciences, and of those included in the medical curriculum in particular, makes uniformity of standard difficult or impossible of attainment, and therefore it becomes necessary to alter the methods of examination so as to allow for the varieties that are necessarily presented by men trained under different systems and in contact with different spheres of observation. The examination, therefore, should not aim at finding out what a man *does not* know so much as at the discovery of what he *does* know and of *how* he knows it—the kind of knowledge and the reality of acquirement being of far more consequence than the mere amount or extent of knowledge. An examination in the present day is worse than useless unless it tends towards the encouragement of real knowledge as opposed to cramming. This error is to be avoided in two ways—first, by confining the expectations of the examiner to what may reasonably be expected m the pupil in respect of the knowledge of books and lectures;

and, second, by extending as much as possible the opportunities given to the student of showing real knowledge in the presence of actual facts. Hence the value of those practical methods of examination which have been introduced of late years, and which are, to a great extent, superseding the older methods. Dr Gairdner dissented from the doctrine that teachers should not be examiners. He had no desire to see the teachers the sole examiners, and on the contrary heartily sympathized with the movement which the Universities are making, and which the University of Glasgow has only been prevented from accomplishing by want of funds, viz., the increase of the number of non-professorial examiners. But he could not for a moment assent to the idea that the teachers ought to be disqualified as examiners. On the contrary, the influence of the teacher over the examination was most beneficial, inasmuch as he alone knew what it was reasonable to expect from the student, having regard to the ground over which he had travelled in his instructions. But further, when the examination is made practical and is conducted in the presence of actual facts—*e.g.*, in the laboratory, in the dissecting-room, in the hospital—it is possible to make even a single case or a single object of any kind the ground for an examination which shall, to a large extent, test the knowledge of the student over a whole range of principles common to that and other objects. In particular, there is no single subject in medicine, and almost no single case of disease, that does not so connect itself with other subjects and cases as to form a fitting and adequate test of almost all elementary knowledge, and also of the mode in which the man has acquired what he knows. Applying this to medical education, Dr Gairdner reminded his hearers that education was not merely information. It is quite true that information must be acquired in a certain measure either by books or lectures, or both together. A certain amount, it may be said, even of cramming—that is to say, of memory-knowledge—is indispensable. But what is far more indispensable is the acquisition by the student of the power of observing, reflecting, turning to account all the opportunities of real knowledge that are presented, and the increase of these opportunities as much as possible. The proper function of lectures is to give life to the doctrine and the knowledge that may be derived from books—to inspire it, as it were, with the personal element that arises from the teacher and the student knowing each other, and being in the habit each of appealing to actual fact directly. Hence Dr Gairdner argued that every lecturer ought to have his practical department—that he should not merely speak to the students in the class-room, but be associated with them in the work of personal observation; and he argued that lectures, when thus reinforced, would thus become even more indispensable as science extended, because the mass of facts and doctrines would require more and more the guiding spirit and life derived from personal instruction.

PUBLICATIONS RECEIVED.

A. M. D.,—The Army Medical Service in the Past and Future. London, 1875.

Professor M'Call Anderson,—Inaugural Address, 6th Nov. 1874. Glasgow, 1874.

Atlas of Portraits of Diseases of the Skin, 14th fasciculus. London, 1875.

B.,—Facts about Breadstuffs. Glasgow, 1875.

C. Binz, M.D.,—Das Chinin. Berlin, 1875.

Braithwaite's Retrospect, vol. lxx. London, 1874.

Dr Julius Braun,—Curative Effects of Baths and Waters. London, 1875.

Prof. Andrew Buchanan,—The Forces which carry on the Circulation of the Blood. London, 1874.

Prof. George Buchanan,—Address on Clinical Surgery. Glasgow, 1874.

Charles A. Cameron, M.D.,—Manual of Hygiene. Dublin, 1874.

Fleetwood Churchill, M.D.,—Diseases of Women. Dublin, 1874.

John Francis Churchill, M.D.,—Consumption and the Hypophosphites. London, 1875.

D. Gesualdo Clementi,—Sulla Medicatura Antisettica alla Lister in Chirurgia. Catania, 1874.

Clinical Pocket-Book. London, 1874.

Edward W. Cox,—Heredity and Hybridism. London, 1875.

W. Howship Dickinson,—The Art and Science of Medicine. London, 1874.

W. Howship Dickinson, M.D.,—Diseases of the Kidney. Part I., Diabetes. London, 1875.

Horace Dobell, M.D.,—On Winter Cough. London, 1875.

J. Matthews Duncan, M.D.,—Mechanism of Natural and Morbid Parturition. Edinburgh, 1875.

William Eassie,—Cremation of the Dead. London, 1875.

David Ferrier, M.D.,—National Medicine. London, 1874.

Cornelius B. Fox, M.D.,—The Disposal of the Slop Water of Villages; Water Analysis. London, 1875.

Thos. W. Grimshaw, M.D., J. Emerson Reynolds, F.C.S., Robert O'B. Furlong, and John W. Moore, M.D.,—Manual of Public Health for Ireland. Dublin, 1875.

Francis S. Haden, F.R.C.S.,—Earth to Earth. London, 1875.

Edward Hamilton, M.D.,—The Present State of the Army Medical Service. Dublin, 1875.

C. H. Jones and E. H. Sieveking, M.D.,—Manual of Pathological Anatomy. London, 1875.

James Keene, F.R.C.S.,—On Defective Hearing. London, 1875.

T. R. Lewis and D. D. Cunningham,—Cholera, Microscopical and Physiological Researches, 2d series. Calcutta, 1874.

T. R. Lewis,—The Pathological Signification of Nematode Hæmatozoa. Calcutta, 1874.

Angus Macdonald, M.D.,—Note-Book of Materia Medica. Edinburgh, 1875.

R. A. Macfie,—The Patent Question in 1875. London, 1875.

E. D. Mapother, M.D.,—Lectures on Skin Diseases. Dublin, 1875.

J. L. Milton,—On Spermatorrhœa. London, 1875.

Fessenden N. Otis, M.D.,—On Reflex Irritations throughout the Genito-Urinary Tract. New York, 1875.

Thomas B. Peacock, M.D.,—General Index to the Transactions of the Pathological Society of London for vols. xvi. to xxv. London, 1875.

Dr Max von Pettenkofer,—Cholera. London, 1875.

Th. Ribot,—Heredity. London, 1875.

Salt and Son,—Medico-Electric Apparatus, and how to use it. London, 1875.

Prof. M. Schiff,—La Pupille considérée comme Esthésiomètre. Paris, 1875.

Walter G. Smith, M.D.,—Commentary on the British Pharmacopœia. London, 1875.

John Kent Spender, M.D.,—Therapeutic Means for the Relief of Pain. London, 1874.

Dott. Ottavio de Stefano,—La Limonea Chloroidrica, Sperimentata utile e sicura per Abortire all'istante il Cholera. Milan, 1874

Transactions of the American Ophthalmological Society. New York, 1874.

Transactions of the American Otological Society, Seventh Annual Meeting. Boston, 1875.

James Tyson, M.D.,—Guide to the Practical Examination of Urine. Philadelphia, 1875.

Professor William Turner,—Introduction to Human Anatomy, Part I. Edinburgh, 1875.

W. W. Wagstaffe, F.R.C.S.,—On Transfusion and the Selection of Fluids for Injection into the Veins. London, 1875.

Haynes Walton, F.R.C.S.,—Practical Treatise on Diseases of the Eye. London, 1875.

M. A. Ward,—Outlines of Zoology and Comparative Anatomy. Dublin, 1874.

Francis H. Welch, F.R.C.S.,—The Nature and Varieties of Pulmonary Consumption among Soldiers. London, 1874.

Samuel Wilks, M.D., and Walter Moxon, M.D.,—Lectures on Pathological Anatomy. London, 1875.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Contributions to the Determination of the Diminution of the Uterus after Delivery.* By AR. SERDUKOFF, M.D., formerly External Assistant to the Obstetrical Clinic at Moscow.

(*Communicated to the Obstetrical Society of Edinburgh by* Dr MATTHEWS DUNCAN, 10*th February.*)

§ I. PARTURITION, as is well known, consists of a series of uterine shrinkings or throes, which gradually expel the child. Each pain is accompanied by a diminution of the blood-supply to the muscular substance of the uterus. Whilst the uterine muscle works, its nutrition is greatly disturbed by the contractions compressing the blood-vessels, and thus interfering with the vascular supply; the muscular substance itself is destroyed—the products of that destruction being absorbed—and the uterus is diminished in size. This shrinking of the muscular substance of the uterus is observed, not only during labour, but also after delivery. This diminution is called involution of the uterus. The object of my paper is to examine in what manner the uterus is diminished under the different circumstances which occur in the puerperal state. The muscular substance of the womb of a newly delivered woman is absorbed; from its external layers new muscular tissue is developed; and, finally, a new uterus is formed. Instead of the old uterus, which weighed, immediately after labour, from two to three pounds, we see, at the end of six or eight weeks after delivery, an organ whose weight varies from one to one and a half ounce—thirty to forty-five grammes. A reference to the Tables given at the end of this paper will show that, during involution, the uterus diminishes in length and breadth in an equal ratio (§ VIII.). The body of the uterus diminishes in size more rapidly than the cervix. Twelve days after delivery, the uterine body is $3\frac{2}{5}$ths (1 : 0288) larger than the cervix,[1] but at the end of the puerperal period the proportion is as two to one.[2] The time required for the completion of involution is variously stated by authors. Fabricius ab Aquapendente[3] holds fifteen days to be sufficient; Deventer,[4] judging from one case, puts it down at eight days; Smellie[1] gives from eighteen to twenty days. More recent authors, although not agreeing as to the exact time required for the completion of this process, greatly extend its limits. Velpeau[2] gives it as varying from five to six or eight weeks; Krause, five weeks; Hohl and Spiegelberg, eight weeks; Schroeder, six to eight weeks; whilst Scanzoni extends it to as much as sixteen weeks. From these statements, then, we perceive how widely opinions differ on this subject—even in recent times, authors varying from six to sixteen weeks. From my own investigations, which are not numerous, I am inclined to believe that the period varies from eight to ten weeks.

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[1] Lott. Anat. und Physiolog. des Cerv. Uter., 1872, p. 109.

[2] Sappey, Traité d'Anat., 1864, p. 664.

[3] De Formato Fœtu, 1600, p. 142.

[4] Obs. Import. sur le Man. des Accouch., traduit par T. T. Brumer d'Ablancourt, 1433, p. 42.

§ II. Ancient authors were undoubtedly aware of the fact that the uterus diminished in size after delivery (*vide* Fabricius), but Deventer[3] was the first who drew attention to the gradual manner in which this occurred, and to the variations in different cases. Smellie says that the womb of a primipara expels clots with greater force than that of a multipara.

Until the present century very few authors, and those very cursorily, have alluded to the subject of this paper. A British physician, Burns,[4] says that the diminution of the uterus after delivery, is a result of the absorption of muscular tissue. Wieland[5] was the first who tried to determine the extent of the diminution by measuring the length and breadth of the uterus after delivery; the one dimension being taken from the upper part of the symphysis to the fundus uteri, while the second is obtained by measuring between the most prominent lateral parts of the uterus. He supposed that up to the seventh day there was a daily diminution in size of from one to one and a half centimetre, and after that of from a half to one centimetre. At the tenth day he describes the uterus as being situated behind the symphysis pubis, and therefore beyond the reach of measurement through the abdominal wall. But Wieland's results are not faultless, he having first neglected anteflexion (vide § VI.); and, second, having failed to notice the regularity of the involution (vide §§ IV. and VI.). On turning to German literature, we find that Schneider[6] has worked at this subject. This author has measured the uteri of 111 women after delivery. He divided his cases into groups, placing those who suckled their children into one, multiparæ into a second, those delivered before the full time into a third, and so forth. But on referring to these divisions we find sources of error which they do not exclude; for instance, some of those delivered prematurely did not suckle their children, and the involution of their uteri was prolonged. Now we do not know whether this was the result of not nursing or of premature delivery, or of both causes.

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[1] A Treatise on the Theory and Practice of Midwifery, 1779, pp. 355-9.

[2] Traité Complet de l'Art des Accouch., 1835, t. ii. p. 613.

[3] "La matrice continuellement tend a se resserrer," *l.c.*, pp. 41-2.

[4] Principles of Midwifery, etc., 6th edit., 1824.

[5] Etude sur l'Evolution de l'Uterus pendant la Grossesse et sur le Retour de cet Organ a l'Etat Normal après l'Accouchement, 1858.

[6] Monatsschr. für Geburtsk., Bd. 31, Heft v. p. 357.

Wieland and Schneider both neglect a circumstance of immense importance, namely, the duration of labour. We are *a priori* led to pay particular attention to this, because prolongation of labour has some value with reference to the size of the uterus immediately after delivery, and influence on its diminution in the days immediately following delivery (see § X.). Then Hecker, Buhl,[1] and Scanzoni,[2] propose to determine the involution by measuring the organ in those women who died at different times after delivery. From the tables of these authors I have taken those cases where death did not apparently arise from disease of the sexual organs, and where, consequently, the uterus has undergone involution almost normally. I have thus got twenty-three cases, including two of my own. From these it is difficult, even if the labours appear to have been at the normal term, to make out a difference from day to day in the diminution of the uterus. For instance, one uterus on the third day after delivery had a smaller size than another on the ninth day. In order to determine the rate of involution in this way, it will be necessary to gather many hundreds of cases, so that individual peculiarities may be effaced.

In our Russian literature there is reference to this subject in the work of Sutugin,[3] who had copious material for his observations. But my countryman, aiming at a very general result, does not classify the puerperal women in the needful categories; and, therefore, we cannot see how the influence of various conditions of the puerperal state acts on the involution.

§ III. Opinions vary much with regard to the regularity of the process of involution. Simpson[4] and Pfannkuch[5] assert that it is a regular one. French authors deny this, but they do not attempt to explain the cause of the deviation from regularity. It is, of course, a difficult matter to demonstrate a law bearing on this point, because the physical and chemical changes which take place in the uterine muscle are variously affected by external influences acting on different women in diverse ways during the lying-in period. Thus, we may observe in healthy women with healthy uteri, a considerable difference in the progress of uterine involution. An apparently healthy womb may go through the process of involution less naturally than a diseased one, provided the former is influenced by conditions unfavourable to the proper performance of this function, whilst the latter is placed under circumstances highly propitious. Attempts have been made to determine these circum-

[1] Klinik der Geburtsk., p. 86.

[2] Lehr. der Geburtsk., 4 Aufl., 1 Bd., p. 356.

[3] A Medical Account of the Obstetrical Establishment at St Petersburg, 1872, p. 150. (Russian.)

[4] Clinical Lectures on the Diseases of Women, 1872, p. 587.

[5] Archiv für Gynaecolog., Bd. iii. p. 372.

stances. Schneider, however, is the only author who has endeavoured to do so, and he has been unsuccessful.

§ IV. My investigations into the involution of the puerperal uterus, although strictly of a scientific nature, are of considerable practical interest. This process has not been, as yet, well studied. There is no monograph of value on the subject, and the text-books afford little information. There are chapters on involution of the uterus, but as to the amount of diminution in length and breadth, at different times of the puerperal period, they are silent. Schroeder,[1] like Schneider, says that involution does not proceed so regularly in primiparæ as in multiparæ. But what is meant by the term regularly? A uterus undergoing involution possesses two independent properties which influence it; the first is shown by a quickness of diminution, the second by a regularity or equality of that diminution. For instance, two uteri may diminish in size, on an average, 1-10th daily. One of them may diminish daily exactly this 1-10th, whilst the other diminishes to the extent of 1-16th of its size on the first day, and 1-7th on the second, etc. Here we have the same *rate*, but not the same regularity of diminution. Now we do not know what Schroeder understands by the term regularly. Does it express a medium normal quickness of the process, or an actual equality or regularity of it? Moreover, the relation between involution and after-pains is not explained in the text-books. Besides the interest attached to the study of this subject for its own sake, other circumstances, which have important theoretical and practical bearings, induced me to undertake these investigations.

The pathology of puerperal fever has long been a subject of discussion amongst authors. The variety and complexity of the circumstances under which this dangerous disease may arise, have greatly increased the difficulty of its elucidation. Such circumstances may be external to the patient, or they may affect directly the sexual organs. From this, it is evident that it is most important to make out, as far as possible, the phenomena of natural as well as of morbid puerperality. Having fixed data for comparing a morbid organism with a healthy one, we can note the primitive morbid processes in the sexual organs; and afterwards we may, by careful study of the external pernicious agencies, be able to arrive at a solution of an interesting question, whose practical application may benefit one-half of mankind.

There are several remedies in the Pharmacopœia which have the reputation of being able to influence the contractility of the uterus; these are, ergot of rye, borax, Indian hemp, savin, cinnamon, digitalis. With the exception of the first mentioned drug, however, their efficacy is very questionable. The incompleteness of our knowledge with regard to the value of so-called oxytocics, ults from the difficulty of determining their influence on the patient. I think much might be done in this direction by experimenting with these drugs during the puerperal period, and noting their effect on the involution of the uterus. We take for granted that the innervation of the uterus is little, if at all, affected by varying conditions of health of the organ. It may show itself by activity of the excitant nerves which strengthen uterine action, or of inhibitory nerves. Those remedies which excite the sympathetic or enfeeble the sacrals, may cause contraction of the uterus whether a woman be pregnant, recently delivered, or a virgin; whilst those remedies which act in an opposite manner will diminish uterine contractility.

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[1] *Loc. cit.*, p. 871.

By employing any of these remedies during puerperality, and watching the involution of the uterus, it will be possible, if we use scientific care, to determine with precision its efficacy in regard to the uterine contractility. In this way the merit of some abortives, such as savin and aloes, may be disproved, while that property may be found in others hitherto unknown.

Besides, this determination may help to explain those feverish conditions which may depend on rapidity of involution,—a process involving great destruction of muscular tissue, absorption of the products of its metamorphosis into the blood, and oxidation of the products.[1] The same may be said of the insufficient involution of the uterus, described by Simpson as sub-involution.

§ V. All the measurements of the 150 puerperal women I have made with my own hands, and can vouch for their accuracy. In taking them, attention has been paid to the following points:—1. Before measuring, the woman must empty her bladder. This she must be made to do even although she asserts that she has recently urinated. If she has any difficulty, the catheter is to be employed. 2. The position must be a thoroughly horizontal one. If the anterior abdominal wall is tense, the uterus has a lower position. If abdominal breathing is well marked, the measurement must be taken between inspiration and expiration, strong inspiration pushing the uterus downwards. No cases, where the uterus may have contracted adhesions to structures, which may interfere with its involution, are admitted into my tables. 3. During the first day after delivery, two measurements were taken; the first, from a half to three hours—rarely six—after delivery; the second, some time during the remaining hours of that day. On the following days one measurement only was taken, and that between 10 and 12 o'clock in the morning. 4. The uteri were measured through the anterior abdominal wall by means of a tape divided into centimetres. The length is first determined. It being desired to have the uterus not under the influence of an after-pain, and yet to have the distinct solidity produced by contraction, I measured immediately after producing a contraction induced by rubbing the fundus uteri. Having placed the smaller three fingers of one hand as a tangential plane to the most prominent part of the fundus uteri, with the other two fingers I hold the tape. The middle finger of the other hand is firmly pressed on the superior border of the symphysis, whilst the thumb and forefinger secure and tighten the tape, bringing it into a straight line. Then I measure the distance between the two hands by noticing where, at each end, they cut across the tape. I act in this manner if the axis of the uterus coincides nearly with the axis of the body; but in those cases where the uterus is inclined to one side—to the right, as often happens—and as such an inclination is hardly distinct, except during the first two or three days after delivery, it is indispensably necessary to remove this complicating circumstance. The fundus uteri is to be cautiously pushed towards the mesial line, and left easily lying there, and then the measurement in length is to be taken in the manner which I have just described.

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[1] Schroeder, Schwang. Geburt und Wochenbett, 1867, pp. 174-5.

5. The breadth of the uterus is determined in the following manner. Three fingers of one hand are placed over the most prominent part of one side of the uterus, while the other fingers of the same hand grasp one end of the measuring tape. The other hand is arranged in a corresponding manner over the other side of the uterus, and the tape stretched tightly between the two; and then, by the eye, note is taken of the points of the tape cut by the planes of the fingers tangentially touching the uterine sides.

In this manner it is found that a uterus diminishing in size day by day will have its length as well as its breadth lessened, if involution be going on under favourable circumstances; and the degree of involution will to a great extent correspond with the amount of diminution of the uterine measurements. During the first few days of the lying-in period, the relaxed abdominal wall does not in any way influence the measurements. At any time when obtaining the measurements the abdominal wall is pressed lightly, and offers almost no obstruction to the exact determination of the size of the uterus, and this is particularly true of its breadth.

§ VI. I determine the involution of the uterus by observing a diminution in its size. I shall now consider the feasibility of this, and how nearly the dimensions by my method of external measurement correspond with the true dimensions.

With regard to the breadth, its determination is not affected by variations of healthy conditions (Pfannkuch). In the case of the length of the uterus, three or four days' constipation affects the results, but in all my cases castor-oil was given every two days, and this element of fallacy avoided. Anteflexion of the womb, which takes place during the first few days of the lying-in period, also affects it; and the greater the anteflexion or anteversion, the smaller will the measurement be that is obtained by the method here described, and *vice versa.* These inclinations of the uterus forwards have a causal connexion with the circumstances of puer[illegible]. If all the women that I measured had been in the same circumstances—especially those which have an influence on anteflexio uteri—that factor would be the same in all, and would therefore have no value. In order to escape as far as possible from this source of error, I made strict inquiry into the previous circumstances of all my patients which might have an effect on the amount of anteversion or anteflexion during the puerperal state. I then divided them into groups, according to the knowledge thus obtained, placing into each group those women who have approximately the same degree of anteflexion. The chief conditions which tend to produce anteversion of the uterus during the puerperal state are the following:—1. A greater relative size of the uterine body in comparison to its inferior part; 2. A considerable yielding of the uterine tissue at a line between its middle and inferior part; 3. A tense condition of the round ligaments, most marked in primiparæ; 4. A distended condition of the intestines.

To estimate correctly the length of the uterus, it is necessary to get rid of, as far as possible, those conditions which tend to produce anteversion. With this object in view, I first examined the cases of those women who were delivered at the same date, and whose uteri on the first day after delivery were of the same size; second, I separate multiparæ from the twinners; third, I consider the primiparæ by themselves; and, last, I pay particular attention to the free evacuation of the bladder and rectum. I must here mention, that I attempted to ascertain the length of the uterus post-partum by means of a sound introduced into its cavity. When this was done I also procured, in the same cases, the external length measurement. I have only made a few such observations, and therefore do not intend to say anything very definite with regard to this method.

The length obtained by internal measurement is greater than that procured by the external method, and there must always be a difference between them. This difference should in various days be approximately the same, for the anteflexion does not change during the first days of the puerperal state. Yet the difference is not quite constant, as may be seen by referring to the following table:—

	Minimum.	Maximum.	Average.
1st day,	1·1 centimetres.	7·8 centimetres.	3·5 centimetres.
2d, „	2·5 „	6·2 „	4·2 „
3d „	3·0 „	7·0 „	5·0 „
4th „	3·5 „	5·4 „	4·2 „
5th „	...	...	5·1 „

We cannot altogether explain the difference expressed in this table by referring to anteflexion of the uterus. Many authors, however, assert that, while anteflexion of the uterus is well marked during the first few days after delivery, it rapidly decreases, and that accordingly the difference between the internal and external measurements will also be diminished after these first few days have elapsed. My table, however, seems to contradict this view of the matter, the difference increasing rather than diminishing.

I have already stated that generally the diminution in length and breadth of the involving uterus go on in the same degree, the latter, however, being the more constant. This being so, I shall compare it with both the methods for obtaining the length of the uterus. The results of the external method bear a pretty constant relation to the results obtained by measuring the breadth of the uterus (as may be seen from the tables). The relation between the results of the external and internal measurements being, as we have just seen, not at all constant, it follows that the external mode of ascertaining the length of the uterus is to be preferred to the internal one.

The disadvantages of the internal method of ascertaining the length of the uterus are as follows:—

1. From the curvature of the sound being always the same, and from the fact that with each measurement we have a varying axis of the uterus corresponding to the curvature of the sound, it follows that we do not by this method ascertain the natural length of the uterus as it lies anteflected or anteverted, but only an artificial length as it is straightened out.

2. The sound is not always easily introduced; its point is frequently to be felt with difficulty or not all through the anterior abdominal wall and thick uterine wall, and you can never be certain that at each observation the point touches the same part of the fundus; accordingly, we may have an apparent length which is less than the real one.

3. This method is not unaccompanied with danger, however carefully it may be employed. I therefore prefer the external method, carried out with the precautions before mentioned.

§ VII. In the tables at the end of this paper, the diminution of the uterus and its degree of regularity can be observed. The latter can be better seen by referring to the ciphers. In order to explain the absolute degree of involution by means of the ciphers, I have employed the following method:—Of two measurements made during the first day, I have taken the mean. I thus noted a dimension every day. All these dimensions, from the first day to the last inclusive, I added together, the result of this addition being represented by the letter (s). If the figures thus obtained be divided by the number of days (j), then we get the average dimension during the period ($\frac{s}{j}$). If thus we determine the length, we obtain the average from the fundus uteri to symphysis pubis; if the breadth is made out, then we get the average between two lateral points of the uterus. From a dimension of the uterus obtained on the first day we subtract the dimension obtained on the last day, and divide the remainder (d) by the number of intermediate days ($j—1$). we determine a diminution of the uterus in one day ($\frac{d}{j-1}$).

Therefore, in order to *determine the absolute* degree of involution, we require to know the quantity a uterus is diminished during one day. For this purpose the degree in which the uterus has diminished in one day ($\frac{d}{j-1}$) is divided by the dimension of the uterus during one mean day ($\frac{s}{j}$). $\frac{d}{j-1} : \frac{s}{j}$ is the coefficient of involution (T). For instance, a uterus—

At the 1st day,	12 centimetres.
„ 2d „	10·8 „
„ 3d „	9·72 „
„ 4th „	8·748 „

Here $s = 41{,}268$; $j = 4$; $j-1$ 3. The degree of diminution in length is represented by

$$T = \frac{3·252}{3} : \frac{41{,}268}{4} = 0·105.$$

In this manner the above formula gives the absolute rate of involution. It allows us to compare uteri of different sizes; in other words, those whose relative dimensions are not equal.

I determine the degree of regularity of involution from the most constant dimension, that of breadth, in the following manner:—Let me take, for instance, the involution of the uterus in the case of "thrombus vaginæ" (see Table III.). The degree of involution $T = \frac{1·6}{8} : \frac{119·7}{9} = 0·015$. The daily diminution of the uterus should be $\frac{1·6}{8}$ centimetres = 2 mm., if the process go on regularly. For the sake of comparison, I take two forms of diminution, a real and an ideal one.

	Real diminution.	Ideal diminution.
1st day,	13·5	13·5
2d „	14·0	13·3
3d „	14·8	13·1
4th „	14·0	12·9
5th „	13·2	12·7
6th „	13·6	12·5
7th „	12·7	12·3
8th „	12·0	12·1
9th „	11·9	11·9

I then notice the difference in the involution on the same days between the two forms of diminution. We perceive the following differences: for the first and last last days = 0; for the second, 0·7 ctm.; for the third, 1·7; for the fourth, 1·1; for the fifth, 0·5; for the sixth, 1·1; for the seventh, 0·4; for the eighth, 0·1 ctm. We next determine the average difference, which tests the degree of regularity of involution, and will show how regularly the involution has gone on. In the case alluded to, this regular diminution is $\frac{5·6}{7}$ ctm. = 0·8 ctm. = 8 mm.

Before entering into an examination of my tables, I must explain the plan on which they were constructed. I have tabulated 150 cases. In comparing groups of cases, I selected those in which general conditions were as far as possible the same, as well as the condition the influence of which I was attempting to determine. Attention was paid to age, stature, number of deliveries, length of pregnancy, length of labour, and, where it was necessary, the size of the uterus during the first day. In cases of bad recovery—endometritis—included in those groups, note was taken of the coming on of the disease, the after-pains, the number of rigors and sweatings, the duration of pyrexia, and of the use of remedies. The history of previous confinements was ascertained.

Having grouped my cases in the manner described, one case being in more than one group, I proceeded to consider the question of involution, its rate and regularity in the case of two of the groups where everything was, as far as possible, equal. Most attention was directed to the determination of the breadth. In all my tables the rate and regularity of involution are deduced from the figures obtained during the first few days after delivery. I did not take into account the dimensions obtained in the 6th, 7th, 8th, and 9th days, because the frequency with which they were determined on those days would not in all cases correspond with the tables, and would lead to error in our calculations, except in the case of the women who nursed.

§ VIII. In Table No. I. is shown the involution of uterus in healthy and unhealthy conditions (endometritis). From it we may observe that a healthy womb contracts better than an unhealthy one. The coefficient of involution in healthy women is $\frac{0·103}{0·083}$, the numerator referring to the length, the denominator to the breadth. In women whose recovery was interrupted by the advent of morbid conditions, the coefficient of involution was found to be $\frac{0·070}{0·087}$. The regularity of the involution is represented by the figures 0·4 centimetre in the first case, and 0·3 centimetre in the second. This table, then, shows that involution is carried on better under healthy than unhealthy conditions, and that there is a difference in the degree of regularity amounting to 0·1 centimetre.

We shall now go on to consider whether this gradual diminution of the uterus occurs during the whole of the puerperal period. I have just shown that the uterus is diminished each day to the extent of $\frac{1}{4}$, $\frac{1}{2}$, 1 centimetre or more. Let us suppose that during the whole puerperal period we had an average daily diminution of $\frac{1}{4}$ centimetre, and that the size before involution began was 13 centimetres; then, at the end of thirty-six days, the uterus would measure 4 centimetres. But this does not happen, because we have already seen that the puerperal uterus regains its natural size only after the lapse of from four to six weeks. It appears, then, that ·tion takes place more rapidly during the earlier than during r part of the puerperal period.

In Table No. II. the women in the first group of cases suffered from metritis, and those in the second from parametritis (paracervicitis, Serdukoff). With this difference, the conditions of the women in both groups were as far as possible the same. In the first group the degree of involution is represented by the figures $\frac{0\cdot078}{0\cdot073}$; in the second, the figures $\frac{0\cdot060}{0\cdot048}$. The degree of regularity of the involution is represented in the first case by the figures 0·6, while in the second by 0·3 only. The difference observable in the two classes of cases in this table is said to be due to a greater length of the feverish condition in parametritis; and Schroeder[1] has suggested that the occurrence of serous infiltration may have also to do with it. But this occurs, and probably in a greater degree, in metritis. Without denying that these circumstances may affect the involution of the uterus, I still think that they do not wholly explain the difference noticed in my two groups, and I suggest other explanations below. Inflammatory tumours in the region of the cervix uteri prevent the uterus sinking into the pelvis to the extent that it does in health. In this way, in parametritis, the mean height of the fundus uteri above the symphysis is 11·9 ctm., while in metritis it is only 9 ctm. In such a position, the uterus cannot be irritated so much by the varying conditions of the rectum and bladder as it is when it is more deeply situated in the pelvis. This may explain the insufficient involution, hæmorrhages, etc., which occur in such cases. This view may perhaps be supported by what occurred in a case of "thrombus vaginæ" which I saw in the Obstetrical Clinic at Moscow. This tumour appeared in the case of a primipara delivered at the ninth lunar month, and was noticed soon after delivery; it was situated in the left wall of the vagina, and increased in size up to the beginning of the third day. After that time, it began to diminish in size. The woman was a little feverish, but the uterus was perfectly healthy. This tumour, being on the level of the lower third of the uterus, may have acted on the uterus in the same manner as the paracervical inflammatory swelling alluded to above. Table No. III. shows us that while the thrombus was present the involution of the uterus did not proceed satisfactorily. We also observe that along with the increase in the size of the tumour, there was an increase in the external length and breadth measurements of the uterus. The involution in this case was unsatisfactory in every particular, in length 0·014, in breadth 0·015, and in degree of regularity 0·8 ctm., and the mean position of the fundus above the symphysis was 12·5 centimetres.

This is an instructive case, because the length and breadth of the uterus were simultaneously both increased and diminished, confirming the law of equal involution of the uterus in two directions.

§ IX. The tables indicate that it not rarely happens on measuring the uterus 1½, 3, 6 hours after labour, we obtain smaller dimensions than on measuring it 6, 10, 20 hours after labour.

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[1] Schwangersch, Geburt und Woch., p. 174.

Kilian was the first to draw attention to this reascensio uteri, and thought it bore a close relation to quick labours. Other accoucheurs scarcely mention this in their text-books, and give no explanation of it. Siebold[1] affirms that it does not depend on the filling of the uterine cavity with blood. Lange[2] ascribes it to relaxation of the womb; whilst Schroeder[3] and Schneider[4] draw attention to the influence of a distended bladder. Pfannkuch[5] holds that where it does not depend on blood filling the uterine cavity, distension of the bladder is the cause of it. Of twenty-five cases where measurements were taken before and after catheterization, in thirteen the uterus at the second measuring was found either of equal length as at the first; or, if its length was less, it presented an increase in breadth.

In my cases I noticed a simultaneous increase in length and breadth in 30·6 per cent.; increase in length alone, in 21·8 per cent.; in width alone, in 28·7 per cent. In 80·1 per cent. of all the cases, there was an increase of both, or of one or other of the dimensions. Here the first measurements were taken two hours after delivery, the second eleven hours after delivery. The uterus, then, is well contracted during two to three hours after delivery; then it becomes relaxed; and ten or twelve hours after delivery it shows an increase in size.

In 17·9 per cent. the smaller dimensions were obtained on measuring the uterus for the second time. In 2 per cent. at the second measuring it was diminished in one dimension without changing in the other. Perhaps if the second measurements were obtained earlier, the number of the latter cases would be diminished. It may then be said that the uterus, having contracted immediately after delivery, is often relaxed at the end of twelve hours.

In one half of the cases on which the foregoing conclusions are founded, there was observed a simultaneous change both in the length and breadth of the uterus (30·6 + 17·3 %). In cases where recovery was uncomplicated by disease, this was noticed in two-thirds of the cases (40 + 20 %). During the first half of the first day after delivery, in half of the cases the uterus is best contracted in one dimension; this is quite consistent with a proportional diminution both in length and breadth during the rest of the puerperal period.

This fact suggests that it is superfluous to follow the suggestion of many authors to give ergot of rye in the last stage of labour in order to prevent bad contraction after delivery.

The following facts may help to explain the increase in size of the uterus which occurs after delivery:—

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[1] Lehrb. d. Geb., ii. Aufl., 1854, p. 334.
[2] Lehrb. d. Geb., 1868, p. 279.
[3] Schwang. Geburt und Wochenb., p. 187.
[4] *Loc. cit.*, p. 371.
[5] Arch. f. Gynaek., Bd. iii. p. 372.

1. When the head of the child passes through the vulva, the contractions of the womb are at their height. This activity or strength of contraction probably lasts some hours after delivery, and thus we get the smaller measurements at first.

2. Immediately after delivery the womb is generally kneaded in order to induce its firm contraction. The relaxation which follows may be the result of flaccidity of the uterine muscle dependent on weakness of nervous power.

3. The presence of blood-clots in the uterus immediately after delivery, may irritate and cause contraction of the uterine muscle.

§ X. It is evident that, when a uterus is prematurely emptied of its contents, and having the minor dimensions, it will reach its former size, *ceteris paribus*, in a shorter time than one which had been evacuated after the tenth lunar month.

In Table No. IV. is shown involution of the womb in cases where everything was the same except the length of the pregnancy. In one group labour took place at the end of the 10th month, in the other at the end of 9½ months. In the first the coefficient of involution is $\frac{0{\cdot}029}{0{\cdot}067}$; in the second it is $\frac{0{\cdot}071}{0{\cdot}066}$. The regularity is 0·2 cent. and 0·4 cent. respectively. We see, then, that involution goes on more quickly and more regularly in women confined at the full time than in the prematurely delivered. From this we may deduce, *a priori*, that in delivery at the full time, when the uterus possesses, after delivery, the greater dimensions, involution will go on better than when it possesses the minor dimensions. Indeed, I confirm such a supposition. Table V. shows the involution of the uterus in women delivered at the full time. Those whose uterus is biggest have the coefficient of involution $\frac{0{\cdot}011}{0{\cdot}082}$, the regularity 0·5 cent.; while the others have the coefficient of involution $\frac{0{\cdot}019}{0{\cdot}067}$, and the regularity 0·2, involution going on more quickly, and regularity being more pronounced, in the first group of cases. We see, then, that the greater the size of the uterus the more quickly does involution go on. This agrees with my former statement, that during the first half of the puerperal period involution goes on more quickly than during the second half.

§ XI. In Table VI. is shown the involution of the uterus in cases differing only in the length of the labour. The time is reckoned from the commencement of regular pains, or those which begin to cause progress. The first half of the cases were delivered quickly, on an average the labour lasting 5¾ hours; the other had labours lasting on an average 13¼ hours. I excluded those cases of labour where the uterus had suffered injury during labour. In the first half of the cases the coefficient of involution was $\frac{0{\cdot}029}{0{\cdot}067}$; in the second it was $\frac{0{\cdot}074}{0{\cdot}064}$. The regularity of the former was 0·7, of the latter 0·4. Involution, then, goes on more quickly in women whose labour does not last long than in those who have been delivered after a more protracted labour. On the other hand, the regularity of involution is better marked in the latter. This is dependent on the fact that the uterus of those delivered after a short labour is larger than that of those delivered after a long one, no time being given in the former cases for involution during labour to the extent that occurs when it is protracted. (See Table VI.)

§ XII. Many authors affirm that the after-pains in multiparæ are more violent than those in primiparæ. This may be explained by noting the connexion between after-pains and involution of the uterus.

With regard to this matter obstetricians vary much in opinion. Schroeder[1] and Scanzoni[2] hold that in multiparæ involution of the uterus goes on more regularly than in primiparæ; whilst Joulin, Cazeaux, Wieland, express an opposite opinion. My opinion which I now give is founded on Table VII., which comprises measurements obtained from primiparæ, of young multiparæ, and of older multiparæ. Age and length of labour were the only points in which they differed. The first group comprises women aged 19½ years on an average; the second, delivered for the second or third time, 22½; the third, mothers from four to ten times, 32 years on an average. The length of the labour in the first case was, on an average, 14·6 hours; in the second, 8¾ hours; in the third, 8 hours. The coefficient of involution in the first group is $\frac{0\cdot061}{0\cdot051}$, the regularity 0·2 ctm.; in the second, the coefficient of involution is $\frac{0\cdot084}{0\cdot075}$, the regularity 0·5; in the third group, the coefficient of involution is $\frac{0\cdot060}{0\cdot047}$, the regularity 0·2 ctm. Therefore, notwithstanding a greater size of the uterus and more violent after-pains in multiparæ up in years as compared with younger multiparæ, involution proceeds more quickly in the latter. In primiparæ, involution, although it proceeds more slowly, is more regular than in multiparæ. The regularity in the first and third cases is less than in the second.

§ XIII. It is the general opinion of obstetricians that after-pains are more frequent and violent in women who suckle their children than in those who do not do so. Accordingly, as was *a priori* to be expected, we find that involution of the uterus goes on more favourably among nursing women. Among authors, Hohl,[3] however, denies this. Table VIII. is drawn up to illustrate this point. From it we learn that the coefficient of involution in suckling women is $\frac{0\cdot076}{0\cdot087}$, and the regularity 0·3 ctm.; while in those who did not nurse there are $\frac{0\cdot102}{0\cdot081}$ and 0·5 respectively. This table indicates that during the first five days of the puerperal period involution goes on more rapidly in the non-suckling than in the suckling women. If, however, we do not begin our calculations until the fifth day, when nursing is becoming established, and we go on to the seventh day, we find that the coefficient of involution in nursing women is $\frac{0\cdot096}{0\cdot095}$, the regularity being 0·9 ctm.; in women who did

[1] *Loc. cit.*, p. 171.

[2] Lehr. der Geburts., p. 355.

[3] Lehr. der Geb., p. 1101.

not nurse, the figures are [illegible] and 0·4 ctm. respectively. These figures indicate, then, that nursing till the end of the first week hastens involution, but makes it less regular. This opinion is opposed to that of Schroeder[1] and Schneider, who assert that involution proceeds more regularly in those women who suckle their children.

I shall proceed to consider the relation between involution of the uterus and after-pains. The mass of puerperal women suffer, immediately or shortly after delivery, from sharp pains in the lower part of the abdomen, which are coincident with contraction of the body of the uterus. Many authors have held that after-pains may be regarded as an expression of involution. Thus, Schroeder[2] says the degenerative process (involution) proceeds the quicker as the uterine contraction is greater. An intense degree of contraction is expressed clinically in the form of after-pains. Therefore, the more violent they are, the more does the process resemble an acute fatty degeneration.

Almost all authors, from Mauriceau[3] downwards, hold that after-pains are more marked in multiparæ than in primiparæ. I shall venture to reconsider this question. It is evident that after-pains must vary in character. For instance, the after-pains of a healthy woman must differ from those of one suffering from endometritis; but we do not as yet know how to distinguish between them, if we merely regard their character, which also, however, must vary, according to the conditions under which they are produced. In relation to this point we have to consider the state of the nervous system, the amount of distension of the uterus, the duration of labour, the degree of contraction or relaxation of the different layers of the muscular coat of the uterus, etc. When the uterus is relaxed its cavity dilates, and blood-clots are formed in it, which excite uterine contractions or after-pains in primiparæ as well as in multiparæ.

This term "after-pains" is a term whose meaning is neither limited nor definite, which probably explains the fact I have made out, that there is no definite connexion between them and involution of the uterus.

So far as I know, no one has as yet investigated this point, the relation between the involution of the uterus and after-pains. In order to do this satisfactorily, they must be studied separately.

The majority of authors, including Schroeder, believe that after-pains are necessary for the production of healthy involution. This view may perhaps be supported by the fact that, during the incidence of an after-pain, the womb becomes hard. But I am inclined to dispute the assertion. In many cases we certainly do observe that there is a coincidence between after-pains and a healthy involution of the womb; but in many others we observe exactly the

[1] Lehr. der Geb., p. 171.

[2] Schwang. Geburt, etc., p. 174.

[3] Aphorisms.

opposite; for instance, 1. In nursing women involution goes on from the second to the fourth day more slowly than it does in those who do not nurse, notwithstanding the facts that the after-pains are at that time most pronounced, and that they are more marked in the former class of women.

2. In aged multiparæ involution is slow, and the after-pains violent.

3. Involution proceeds more rapidly in healthy women than in those labouring under endometritis, although the after-pains in the latter are very well marked, forming a considerable item in our diagnosis of the complaint.

From these facts, it is evident that after-pains are not absolutely necessary for the proper carrying on of involution of the uterus, cases being common in which quick and regular involution proceeds in their absence. We cannot, however, say that after-pains ever interfere with the proper involution of the womb, although we do assert that slight tonic contraction of the uterine muscle is all that is necessary. When violent after-pains occur, they are in many cases to be considered as the result of violent contraction of the whole womb, particularly of its middle muscular layer, in which run the bloodvessels and sensitive nerves (?). Compression of that middle layer is perhaps indicated by the painfulness excited by some after-pains.[1]

§ XIV. Many authors trace a connexion between indifferent involution of the uterus and various sufferings within the sexual sphere. Tables I. and II. indicate the connexion between slow involution and diseases of the inner surface of the uterus, its parenchyma, or surrounding cellular tissue. In Table III., we observe that, in the women suffering from parametritis, the measurement first taken indicated a greater dimension than that procured from the second measurement. In § IX. I pointed out that simultaneous changes in size at the first and second measurings were more frequently observed in healthy than unhealthy recoveries. I cannot here enter into detail as to all the circumstances which precede indifferent involution, but shall only mention two kinds which are unaccompanied by suffering during the puerperal period. I allude to the sub-involution and super-involution of Simpson, which is accompanied by amenorrhœa and other phenomena. Schroeder[2] says, with regard to super-involution, that, from an anatomico-pathological point of view, it is difficult to distinguish between a very rapid involution of the uterus and inflammation. From a clinical point of view, this is still more difficult, because very rapid involution of the uterus is accompanied with increase of temperature, which obtains in and is characteristic of inflammation. I have met with two cases of very rapid involution, which, however, were not accompanied by any great increase of temperature. The temperatures in these cases were not so carefully observed as I

[1] Lazarewitch., Lancet, No. 7, 1867.

[2] Schwang. Geburt und Woch., pp. 174-5.

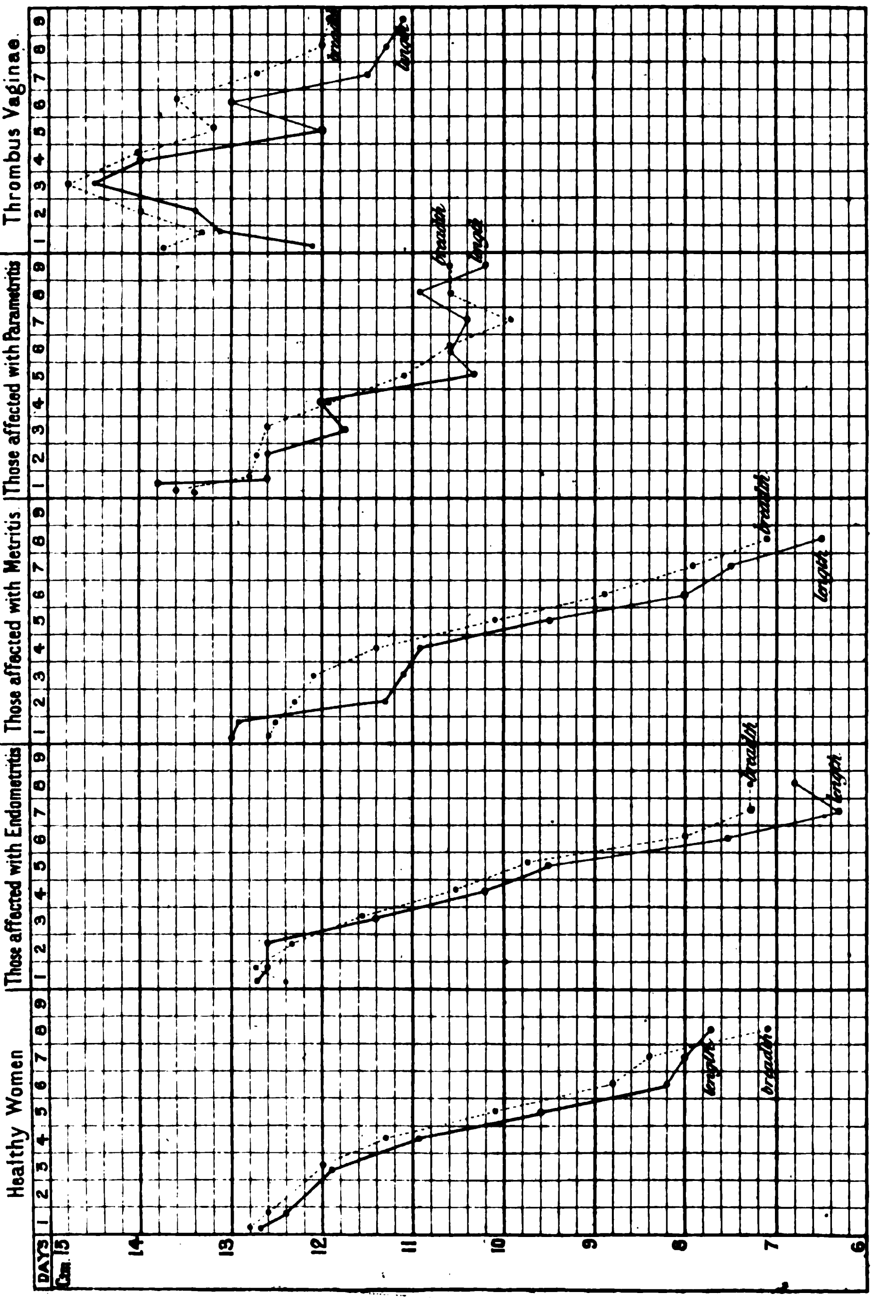
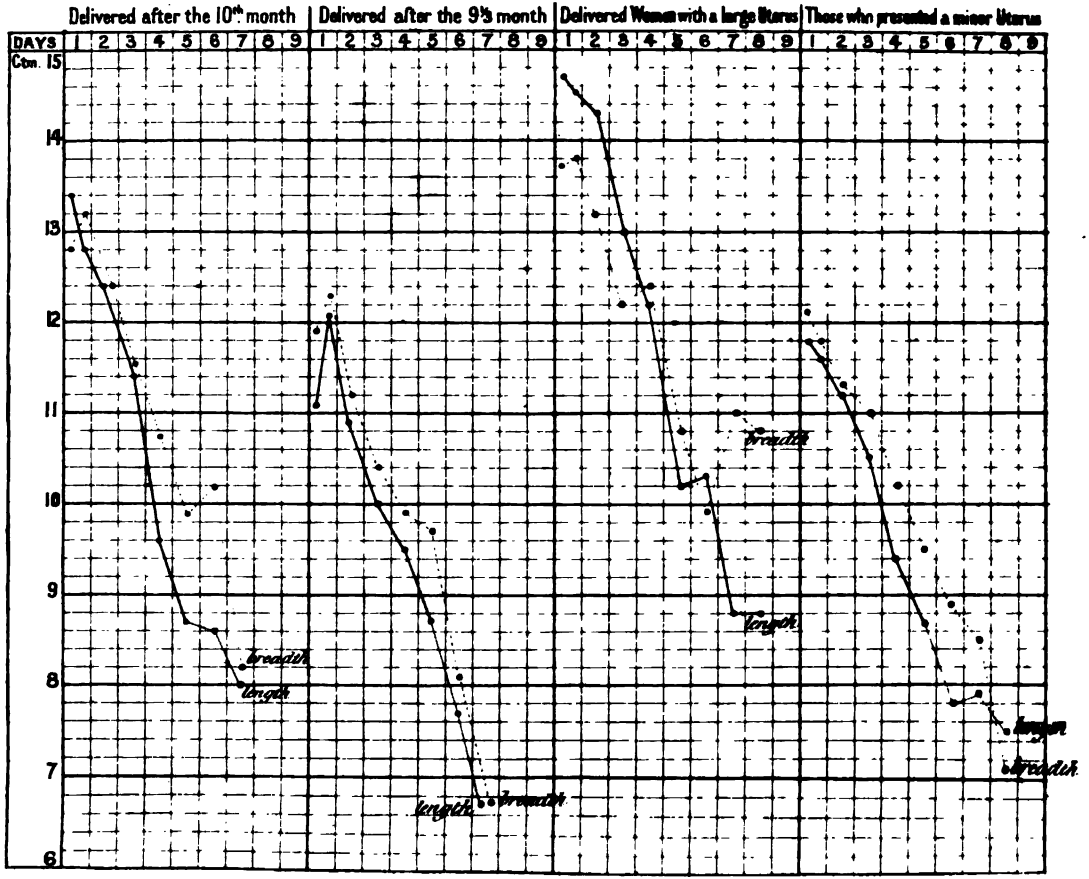


TABLE 1.
Healthy Women
Those affected with Endometritis
TABLE 2.
Those affected with Metritis
Those affected with Parametritis
TABLE 3.
Thrombus Vaginae
DAYS
Cm.
1 2 3 4 5 6 7 8 9
15
14
13
12
11
10
9
8
7
6
length
breadth


Delivered after the 10th month
Delivered after the 9½ month
Delivered Women with a large Uterus
Those who presented a minor Uterus
DAYS
1 2 3 4 5 6 7 8 9
Ctm. 15
14
13
12
11
10
9
8
7
6
breadth
length
length
breadth
breadth
length
length
breadth

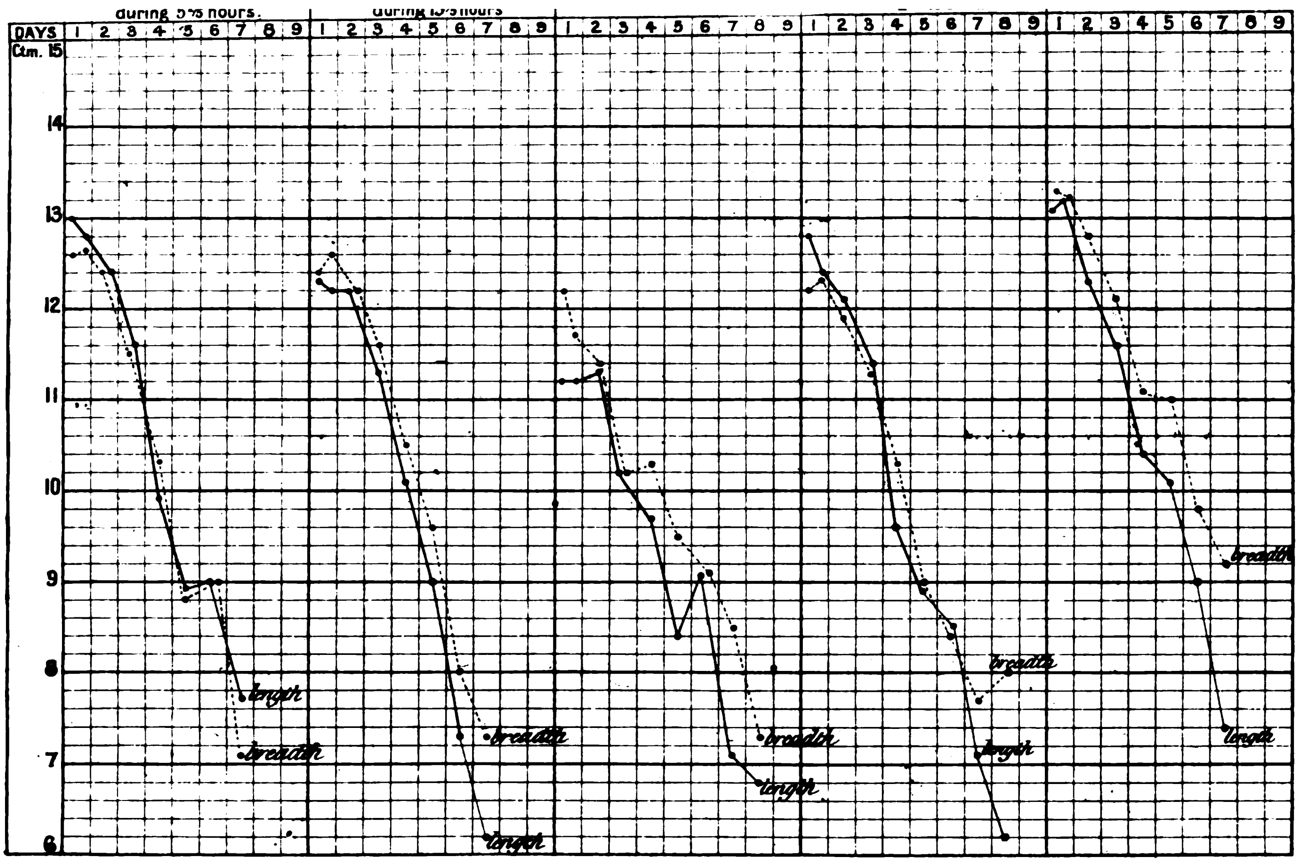
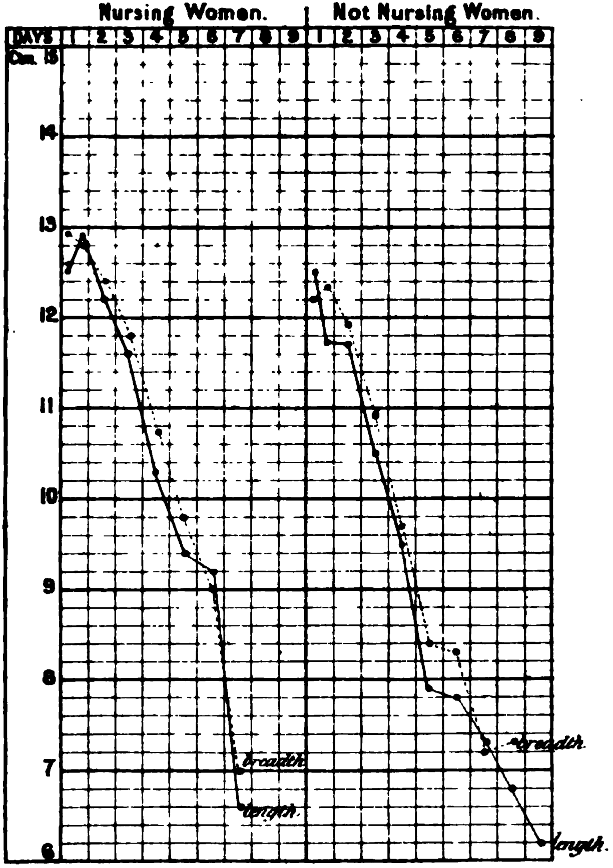

during 5⅔ hours.
during 15⅔ hours
DAYS
1 2 3 4 5 6 7 8 9
Ctm. 15
14
13
12
11
10
9
8
7
6
length
breadth

TABLE 8.


Nursing Women. | Not Nursing Women.

should have liked, and, owing to this, I must not contradict the likely and positive opinion of Schroeder with regard to it.

1. Involution of the uterus goes on more rapidly during the first few days of the puerperal period than it subsequently does.
2. Involution of the uterus of healthy women goes on well and with regularity.
3. Involution, where the uterus is the subject of disease, such as metritis, endometritis, or parametritis, goes on more slowly, and this varies with the amount of disease.
4. The permanent contraction which takes place during the first few hours after delivery is a common occurrence. When it passes off an increase in size begins to take place.
5. In women delivered at the full time, involution goes on more quickly and regularly than in those prematurely confined.
6. Length of labour retards involution.
7. In primiparæ involution of the uterus goes on very regularly, but more slowly than in young multiparæ. In aged multiparæ involution does not go on so well.
8. In women who suckle their children, involution during the first four days does not go on so quickly as in those who do not nurse. But, subsequently, the involution is quicker, though less regular.
9. After-pains are not necessary for a favourable involution; in fact, we are as well without them.
10. In order to determine the involution of the uterus, you should only measure its breadth.
11. Involution of the uterus goes on proportionally in length as well as breadth.
12. Super-involution and sub-involution occur as distinct uncomplicated pathological conditions.

ARTICLE II.—*Cases illustrative of the Principles of Conservative Surgery.* By JOHN AYMERS MACDOUGALL, M.D., Galashiels.

(*Read before the Medico-Chirurgical Society of Edinburgh, 3d March.*)

THE three following cases I class together and bring before you, as fair examples of what conservative surgery can accomplish, and how, as surgeons, we may trust the reproductive powers of the periosteum.

CASE I.—*Acute Necrosis of Tibia—Subperiosteal Resection of entire Shaft.*

In the latter days of August 1873, I saw J. F., a strong, healthy boy, æt. 7, who for some days had been confined to bed with a sore leg. My assistant, who had seen him once or twice previously, thought the case one of erysipelas, but, alarmed by the grave constitutional symptoms, desired me to visit him. I found him certainly very ill. Pulse 150; high temperature; tongue dry and red; quick jerky breathing, and incessant delirium. The right leg, which was drawn up in close contact with the thigh, was red, tense, and œdematous. Careful examination gave evidence of deep-seated suppuration.

In the knee as well as in the ankle-joint, considerable effusion existed, and the least attempt at movement caused great suffering. A free incision in the lower third of the leg over the tibial crest gave vent to a quantity of foul-smelling pus, and the finger introduced into the wound detected bare dead bone, verifying the diagnosis of the case. So grave were the symptoms, and so unsatisfactory all inquiry as to an exciting cause, that the case looked almost pyæmic *ab initio*. Poultices were applied round the limb, and stimulants, with an opiate at night, administered.

For many days there was little change in his condition; the irritative fever threatening quickly to destroy him. Gradually, however, his urgent symptoms became somewhat ameliorated, his tongue became less dry, his pulse, with no diminution of frequency, became better in volume, his temperature fell slightly, and he rested better at night. One distressing and anxious symptom he still, however, retained, that of incessant vomiting; and it was only with the greatest care that sufficient nourishment could be administered.

Temporizing for a few days longer, so little progress was made, and so weak was the patient, that operative interference became imperative. Careful examination with the finger and a gunshot probe, coupled with the fact that effusion existed in the joint at both ends of the bone, left no doubt that the whole shaft was affected. In such circumstances, two modes of procedure presented themselves—amputation at the knee-joint, or subperiosteal resection of the dead bone. I was naturally loth to remove a limb in one so young, whose social condition must render him self-supporting, and who had hitherto made so brave a stand. The contracted position of the leg weighed more than aught else against the conservative operation; for I feared that, allowed to remain so for weeks, with a certain amount of diseased action within the joint, the hope of straightening it was not great. Once, however, under chloroform, I thought the matter could be determined.

On the 13th of September, three weeks from the time I had first visited him, with the assistance of my friends Drs Somerville and Stuart Muir, I proceeded to operate. After having him thoroughly under the anæsthetic, and a tourniquet applied, an attempt was made to straighten the limb; but when a fair amount of success had been attained, the dead bone broke. Making a long incision from the tuberosity of the tibia to within an inch or so of the ankle, I discovered the fracture to be at the upper epiphyseal end—unhappily situated for removal of the bone *en masse*. Following the ure of Mr Holmes, after separating the periosteum and soft tissues, I placed a chain-saw near its centre, and endeavoured to divide it. The saw, however, locked, and so great was the hæmorrhage, that, fearing to lose time, I severed it with a pair of strong cutting pliers. The removal of the lower end was readily accomplished by lateral movement, but a small splinter of the inner malleolus, which had apparently not died, remained. The wound was stuffed with strips of lint, and the limb placed on an ordinary wire splint, bent nearly at a right angle. . The shock proved very great, and it was some time before he quite rallied. An opiate was given him at night, and his stimulant continued.

14th.—Passed a restless night. Pulse 180; tongue very dry; vomiting constantly. There has been a good deal of oozing from the wound. The swing apparatus, which I was anxious to use, he could not tolerate, so sand-bags were employed to steady the limb.

15th.—Rather better; pulse 160; vomiting less. The limb, which seemed a good deal swollen, was not disturbed.

16th.—To-day the bandage was undone, and the lint pledgets removed. His general condition was rather better.

17th.—Wound suppurating freely; general symptoms much improved; the vomiting has ceased.

20th.—Indication of matter forming on the inner side of the knee, a free incision was made, which gave vent to a small quantity of pus, and afforded opportunity for an examination which, as regarded the integrity of the joint, was satisfactory. At many points on the suppurating surface of the wound minute osseous patches are visible, and the general condition of the patient continues to improve.

31st.—From the date of the last entry all has gone well. The wound continues to discharge a large quantity of pus, but his general symptoms are all one could wish. Now that the inflammatory swelling has passed off, the limb proves difficult to manage. The still existing irritation in and around the joint precludes any effort at extension, the result of which is that the upper epiphysis is dislocated considerably from the plane of the shaft. This displacement I found best obviated by having the body of the splint, from a little below the knee, made of tin plate, bulging slightly forward, so that the soft parts were kept considerably above their former level.

For some little time nothing further was done. The progress of the patient was satisfactory. The wound, granulating from the bottom, was gradually filling up, and the parts consolidating.

At the end of a month all irritation in and around the joint having disappeared, I considered extension might be safely attempted. The angle of the splint was reduced, and a firm pad and bandage placed over the knee. Every few days some little alteration was made—some gain effected, and at the end of three weeks the limb was all but straight. Acting upon a suggestion I had noted some time before, the wound, which was now comparatively superficial, was kept open. Strips of dry lint applied to the raw surface, and allowed to adhere, effected this. My object was, by this superficial irritation, to increase if possible the renovating process going on in the deeper parts.

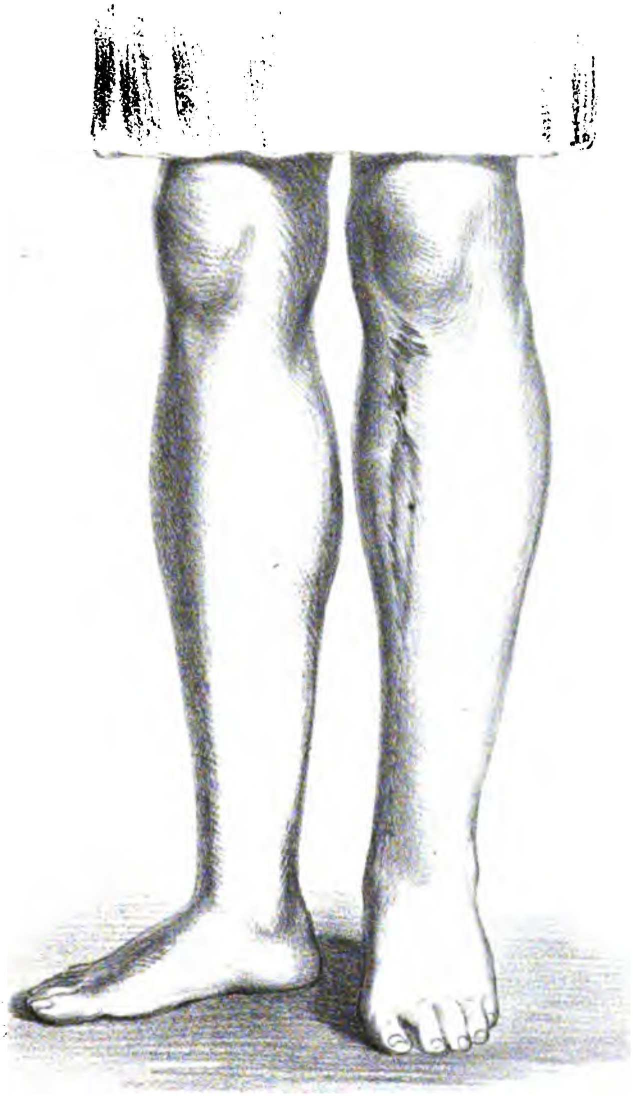
Six months after his operation he was first allowed out of bed. Restless and active as he was, I feared mischief might have befallen him had he ventured sooner; but as a preventive to his walking, the splint was kept applied. Spite of close watching, he managed at times to get rid of it, and bear his weight upon the yet too weak limb. Evidence he bears now of his indiscretion, in a certain amount of antero-posterior bending.

At this time measurement gave evidence of quite half an inch of shortening, and the bone slightly bent did not improve the appearance of the limb. A raw surface was still maintained, and a fixed apparatus applied so as to prevent further harm. Inspection from time to time showed the bone increasing in thickness and strength, and gradually the superficial sore was allowed to close. In the end of May, nine months from the date of operation, all restrictions were removed, and since that date till now, he has been as active, as foolhardy, as his fellows.

24th Nov.—The measurements to-day make the sound limb, from the upper border of the patella to the sole, 12 inches; the bad one 11¾; only a quarter of an inch in difference. The movements of the knee-joint are perfect; and were it not the deep hollow between the hamstring tendons and the femur — the result, I imagine, of the partial dislocation of the epiphysis—and the much larger shin-bone, it would be difficult to judge between the limbs. The new bone is considerably thicker than its fellow by three-quarters of an inch at the upper, and five-eighths at the malleolar end.

By no means a rare disease, acute necrosis is often one of the most disastrous in its results with which the surgeon has to contend. The primary symptoms are generally grave in the extreme, and death within weeks, or even days, from pyæmic lesion is unfortunately too common. Should the patient happily survive the irritative fever, and the suppurative stage be entered on, operative interference in many instances becomes imperative. As a rule, this interference has hitherto taken the form of amputation; and assuredly, in many cases it seems all one can do. The long convalescence resulting from the tedious separation of the dead bone, the many risks attending that separation in the form of pyæmia, hæmorrhage, the implication of neighbouring joints—even the ultimate imperfect reproduction of the lost tissues—all seem to render it the best and safest procedure; but such a case as the one narrated, proves that, dreading as we may an expectant treatment, ve yet another method besides removal of the limb.

nowledge of the opinions of Mr Timothy Holmes, I am



I.- SUBPERIOSTEAL RESECTION OF ENTIRE SHAFT OF

indebted for the mode of treatment adopted in this case. With very much to say in its favour, he still has objections to it—the risk of hæmorrhage he points out; and in such a class of cases this is a most anxious consideration. The bloodless method of Esmarch will, I believe, in the majority of instances overcome this.

The shortening of the limb and immobility of joints which in his experience, as narrated in his work on the "Surgical Diseases of Children," has occurred in every case of subperiosteal resection, is *not* always a certainty. Neither annoying result does the present case show. More than ordinarily fortunate I may have been in meeting with such a one; but, looking back upon it, I believe that the greatest seeming objection to the conservative measure—the contracted position of the leg—proved a boon in the end. With the limb upon a very marked incline, the danger of pus burrowing in the neighbourhood of the knee-joint was less; and the weight of the foot, slight though it might be, would aid in a measure as an extending power. Ere the knee was got to its proper level, and the limb straight, the parts were so firm and consolidated, that the risk of shortening must have been less.

How much the preserving a raw discharging surface had to do with the formation of the new bone, is difficult to determine; it seems, however, by no means unreasonable that increased vascularity and rapid cell-formation in the superficial layers of a part should intensify the activity of a similar process at a greater depth.

The apparatus used—an ordinary wire splint with a tin-plate backing for about two-thirds of its length—I found answered all requirements; the only objection to its use was its extreme lightness, and the fact that the patient could remove it so readily.

The strongest impression I have in connexion with the whole case is, that the boy's radical improvement dated from the operation; and my belief is, that had I elected, in preference to amputating his limb, to stand by and aid nature, his life would have been lost. In anxious circumstances, it is a most admirable procedure; and the result, necessitating though it does constant attention on the part of the surgeon, is gratifying in the extreme.

CASE II.—*Compound Fracture of Lower End of Humerus, implicating the Elbow—Primary Subperiosteal Resection.*

On the forenoon of Sunday, the 31st of May last, W. G., a strong active boy, æt. 5, while riding the rail of an outside staircase, overbalanced himself, and fell a distance of nearly sixteen feet. In his fall, his right arm seems to have come in contact with the pointed top of a low palisading. When I visited him, the symptoms of shock, marked at first, had pretty well passed, but he made great complaint of pain in the arm.

Examination showed very severe injury. Both bones of the forearm were broken, and, from a wound about two inches above the bend of the elbow, the upper part of the fractured hum protruded. Broken as it was a little above the condyles, the flat expanded portion of the bone, driven through the biceps, had all but divided it, just above its tendon of insertion.

The surface of bone was splintered and uneven, and the periosteum stripped back. An attempt at reduction failed; and reflecting that, from its close proximity, the joint was in all probability involved, that in any circumstances union of a fracture in such a situation was almost sure to end in immobility, I determined to resect the elbow. This was done by the single linear incision, and revealed, as was suspected, extensive injury to the joint. Finding that still a certain amount of force was required to reduce the extruded humerus, I enlarged the wound in front, and, after carefully separating the periosteum, I divided the bone as low down as possible, close, however, upon the medullary canal.

Once finished, appearances were far from satisfactory. With a somewhat ragged wound in front, nearly as long as the posterior incision, with the entire muscular tissue divided, with nought intact but the vessels and nerves, the result seemed very doubtful. The amount of bone removed was necessarily very great, but the fact that the periosteum had been left rendered me more sanguine. Reducing the fracture of the forearm, side splints were applied, while pads were placed on each side, the elbow supported by a light bandage. The wound was dressed with a solution of carbolic acid.

1st June.—Has passed a fair night. Pulse 90; slight swelling of the upper arm.

2d June.—Very feverish; had a bad night. Pulse 140; great swelling of the arm. The bandage was undone, and, as the tension in the neighbourhood of the wound was considerable, one or two sutures were removed. Tepid water dressing applied.

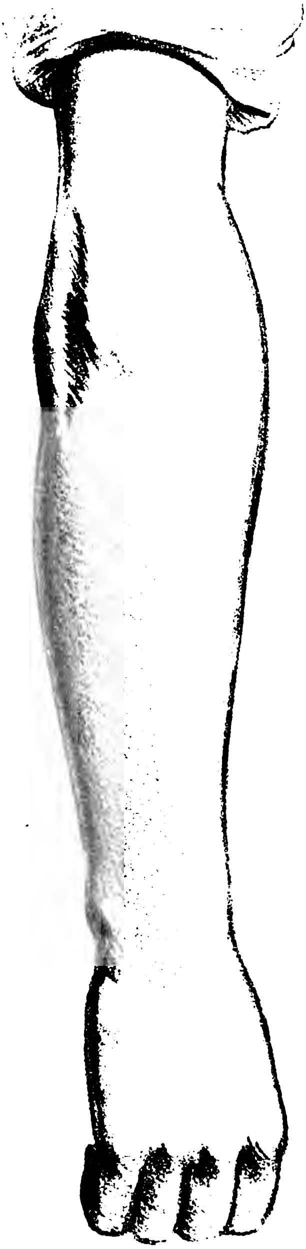
3d June.—Still very ill; arm enormously swollen. Pulse 140; very delirious. As he was necessarily restless, I placed the limb on an inside leather splint extending the whole length of the arm, the forearm being supported by an additional back splint.

4th June.—To-day there is no improvement. Symptoms much the same. Suspicions of osteo-myelitis were fortunately not verified on examination of the bone. All sutures have been removed, and, as the wounds are sloughy and unhealthy-looking, charcoal poultices have been applied.

5th June.—Better to-day. Pulse and temperature fallen. Delirium, which for the last three days has been incessant, is diminishing, and the local conditions seem improved.

12th June.—Since last entry, he has gone on well; suppuration once freely established seemed to bring relief. On removing the inner splint, the joint is "flail-like" in the extreme.

Six weeks after the operation the splint was discontinued, and ads used. So supple was the part, that passive motion as against ening seemed very unnecessary. With a view, however, of



II.- PRIMARY SUBPERIOSTEAL RESECTION OF ELBOW

strengthening the muscular tissues, it was employed gently at intervals of two days. In the end of August all dressings were removed; the end of the humerus had been wonderfully reproduced, and the artificial joint so useful, that the natural motions were intact. Careful and repeated measurements showed absolutely no shortening of the limb.

CASE III.—*Contraction of the Arm, with Anchylosis of the Elbow, the result of severe Burn—Treated by Subperiosteal Resection.*

In the end of August last, M. G. first came under my care. She was a pale, sickly-looking girl, about twelve years old, stunted for her age. Four years ago, by falling in the fire, she sustained a severe burn of the right side of the body, the arm suffering to the greatest extent. For many months she lay in an English provincial hospital, where all seemed to have been done to avert contraction, but unfortunately without success.

When I first saw her the arm was bent at nearly an acute angle, a dense cicatrix of great thickness occupied quite one-third of the anterior aspect of the limb, and over the olecranon was a large fungus-looking sore, at the bottom of which spongy bone could be detected. This sore she complained much of as a seat of great pain, aggravated at night. With such a condition of parts the usefulness of the limb was all but lost; and this, coupled with the fact that the constant pain and irritation wearied her, led the surgeons under whose care she first was to advise amputation as all that could be done. Most unwilling to submit to this extreme measure, she was on her way to the Edinburgh Infirmary, when, through the kindness of Dr John Smith, of Wemyss Place, she was brought under my observation. Finding that the cicatricial contraction of the limb and the anchylosed condition of the joint prevented all useful movement, that the seat of pain was in the olecranon, the substance of which was thoroughly diseased, I proposed resection as a conservative measure. This, with the kind assistance of Dr Smith and Dr Jeffrey of Jedburgh, was performed on the 2d of September. The tissues on the posterior aspect of the joint forming the flaps were unfortunately part of the general cicatrix, and the osseous tissue seemed more fused together than ordinarily anchylosed. Removing a large triangular portion by means of strong cutting pliers, complete division was readily effected, the ends of the bone cleared; and, well aware that in such circumstances it was difficult to remove too much bone, I cut away quite three inches. The shortened, contracted condition of parts rendered free removal absolutely necessary. The limb could now be wonderfully straightened, the cicatrix yielding readily; and had it not been for the condition of the superficial textures, matters would have looked well. So thin and attenuated were these, that, lest the stretching of them would do harm, no sutures were introduced. Pads and a light bandage were applie

Next day all looked well; there was a certain amount of healthy action about the wound, and the general state of the patient was favourable. In two days, however, conditions were changed; the thin, imperfectly vitalized tissue forming the flaps sloughed completely away, leaving the divided end of the humerus exposed and bare.

The pulse and temperature rose rapidly, and the condition of the patient was certainly an anxious one. The angle of the limb was altered, and a pad placed upon the lower end of the humerus in order if possible to keep its divided extremity in the deeper part of the wound; but this arrangement was not very successful. As the patient did not improve, and as the amount of sloughing which had occurred left little hope of the bone being covered in, I enlarged the incision upwards, protruded the humerus, and after stripping the periosteum carefully up, divided it nearly two inches higher, opening into the medullary canal. This I had the less hesitation in doing, for, associated with great constitutional disturbance, the exposed end of the bone had begun to look unhealthy, and the periosteum to recede from its edges. The "flail" character was now but too evident. The dense cicatrix in front gave, however, a measure of support, wanting in the majority of cases.

A splint of leather was applied to the inner side, with the usual pads on the upper aspect and in front. From the date of this last operation the general symptoms improved; pulse and temperature gradually fell, the wound slowly contracted, and at the end of four weeks so firm were the parts that the splint was discontinued. Passive movement, which was begun at the end of a fortnight, was steadily persevered in, and the freedom of parts was most satisfactory. As time passed, however, spite of the constant movement to which the joint was subjected by the patient herself, and at my own hands, an amount of thickening occurred over the lower end of the humerus which rendered the mobility unsatisfactorily difficult. This was but too clearly due to osseous formation.

Now (20th January) the parts are healed and consolidated, all pain and uneasiness have gone, and the general condition of the patient is vastly better. Moreover, the appearance and usefulness of the limb are much improved. She cannot quite straighten it, but she has such an amount of flexion that she can raise the hand nearly to the mouth. Knitting and sewing she does readily, so that, though somewhat disappointing in its results, the operation has in many ways been fraught with benefit to her.[1]

Perhaps in the whole range of surgery no operation bears so well the term conservative as resection of the elbow, and every surgeon

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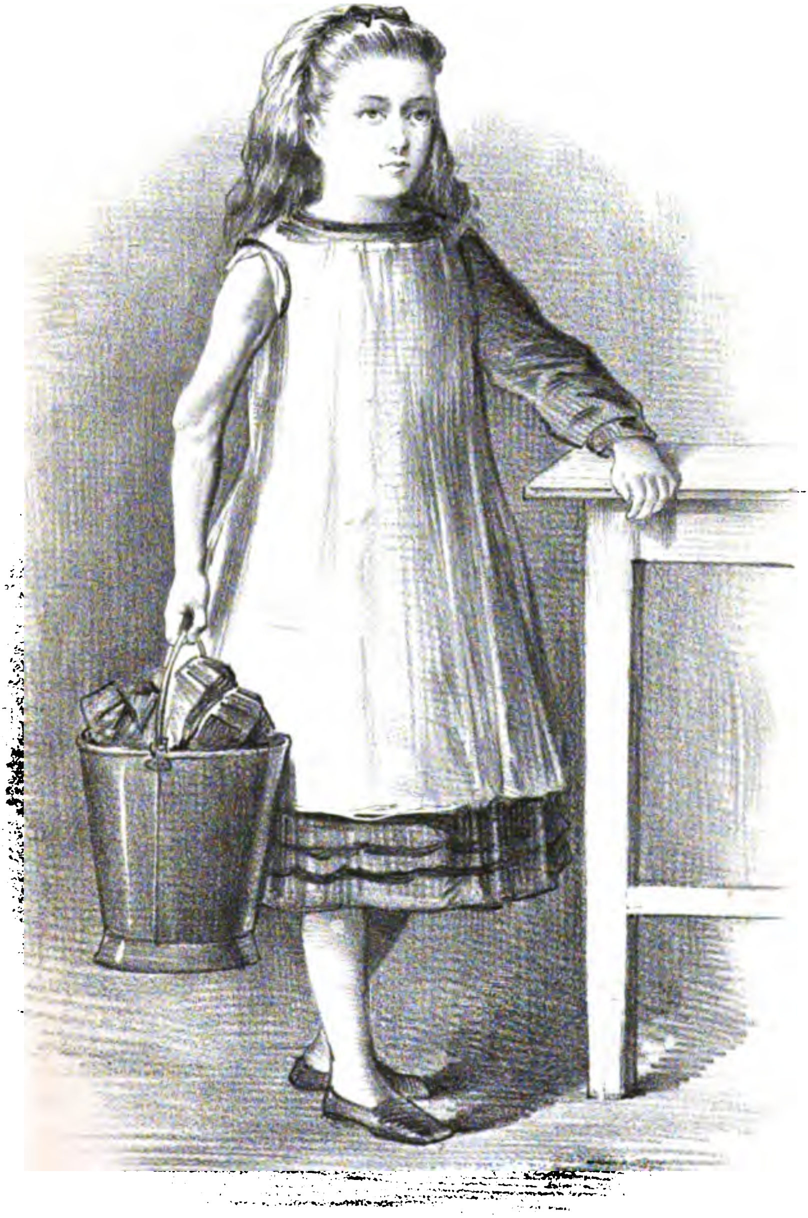
[1] Since this was written I have seen her, and so strong has the arm become, she can now carry a full pail of water up a long stair, and perform almost duties that would be required of her as a domestic servant.



III.- CONTRACTED ARM FROM BURN - SUBPERIOSTEAL RESECTION.

knows to what a variety of cases it is applicable. Those I have just narrated differ certainly from the ordinary, inasmuch as one was a primary resection for injury, the other for a disabling deformity the result of severe burn. Both were performed to a certain extent subperiosteally, and present, I think, good illustrations of the one class of cases in which such a proceeding is beneficial; and of the other, in which it is not only unnecessary, but injurious.

Some surgeons, notably Langenbeck of Berlin, have proposed that the periosteum in all cases of elbow resection should be left. The reproduction of the bone removed, the consequent prevention of shortening of the limb, and the formation of a more seemly joint, are the objects thereby aimed at. But in the great majority of instances this is quite unnecessary. Little periosteum there is to leave, the articular cartilage of the humerus covers in great part what of that bone is removed, and the reproduction of an olecranon process can in no way add to the greater mobility and consequent usefulness of the new joint. In cases of disease, moreover, with carious bone, the periosteum is often so disorganized that its reproductive powers must be all but lost.

Cases of anchylosis demand as a rule free excision of bone—the error of removing too little is by no means unfrequent. The case of M. G. is one in point. Had I removed at my second operation both bone and periosteum, I believe the patient would now in every way have had a better and more thoroughly useful limb. Judging imperfectly the amount of shortening which the burn cicatrix gave rise to, and unaware of the great density of new texture following the sloughing of cicatricial parts, I left a certain amount of free periosteum in order to avoid a weak, flail-like joint. Weeks after the operation was performed, and when the ligamentous union, as regarded mobility, was all one could desire, osseous formation at its upper end, and a consequent impairment of motion, proved the error of the proceeding. And this, in the majority of ordinary instances, will be the annoying result. Subperiosteal resection of the elbow for disease cannot, I believe, be reckoned an encouraging or successful operation. In cases of injury, however, it is somewhat different. We may have an amount of splintering or fissuring of bone demanding removal, such removal entailing great doubts as to the future usefulness of the limb. Happily, however, in such circumstances the periosteum can be readily preserved, its preservation the best guarantee that the lost tissue will reappear. Had no periosteum been preserved in the case of the boy W. G., a too supple union must, I imagine, have resulted. The shaft of the humerus was all but broken in upon—the entire condyloid portion was cut away; and yet within three months of his accident he had regained the full use of his limb.

My own experience has taught me how much in the long-run we may expect from flail joints, and what a gain the very worst of them is over an artificial limb; but where we have a possible means of avoiding such results, it behoves us as surgeons to adopt them; and in many cases of primary resection of the elbow, the subperiosteal method will, I believe, prove valuable and useful.

ARTICLE III.—*Lithotomy in the Service.* By Surgeon-Major WILLIAM CURRAN, A.M.D., Murree, Punjab, India.

No. 883, Private J. M'P., 72d Highlanders, a native of Aberdeen or its vicinity, is 29 years of age, and has eleven years' service, three of which only have been passed in India. He is a married man; of temperate habits, and fair general health; and there is no hereditary history of rheumatism or other constitutional ailment in his family. He states that he has been troubled with symptoms of urinary irritation and pelvic disturbance as long as he can remember, and that these are liable to be aggravated by rapid movement of any kind, protracted walking, or other continuous exercise. They have been greatly intensified since the introduction of running drill, and he has been frequently forced to fall out on parade, and seek relief in the recumbent or prone position. A constant dribbling, with its attendant discomfort, and inability to empty the bladder satisfactorily, have been superadded to these since he entered on the march from Umballa to Peshawur, and all his sufferings had been so enhanced by this as to oblige him to apply for admission, on arrival there, to the wards of his own hospital.

When seen by his own surgeon after the march above referred to, on the 16th of December 1873, there was so much spasm of the urethra as to arrest the flow of urine, and simulate stricture; and he was accordingly admitted under that heading. As soon as the former, however, was subdued by baths, demulcents, and other appropriate treatment, the water flowed more freely, and, being examined, was found to contain a good deal of mucus and some albumen. He also complained of a sense of weakness and pain in the back, and his pasty anæmic appearance appeared to point to the presence of renal engorgement or degeneration. His disease was changed to nephritis; and it was while being treated for this, with negative results, that my attention was first called to him. There could be no doubt as to the gravity of his symptoms, on the occasion of my visit to him; and having ascertained from himself that they had lasted a long time, and were mainly referable to the penis and bladder, I suggested to my colleague, Dr Nash, the propriety of a physical examination. This could not be satisfactorily carried out at the time, owing to his state of irritability and depression, and we accordingly deferred it for the purpose of securing ore quietude and repose. Meanwhile, having satisfied myself that micturated much more frequently by day than during the night, that he never passed water without experiencing a stoppage or suffering pain along the floor of the urethra or at its end, and, moreover, that he had occasionally noticed blood in the urine during the march, I came to the conclusion that he must be the subject of either a fungus of, or a calculus in, the bladder, and I determined to test the matter thoroughly as soon as his condition would allow. This was done accordingly on the 20th of January; but the agony and distress induced by the passage of the sound and its manipulation were such as to neutralize our efforts, and oblige us to bring our examination to a close without arriving at any definite result. Enough, however, had transpired to satisfy me as to the presence of a foreign body, and I said as much to my colleague on leaving the ward, and added, that were the patient a native, I should have no hesitation in operating on him, on the strength of the evidence already obtained.

As the irritation produced by the passage of the sound and its rotation within the bladder was such, on the occasion of our late examination, as to preclude all hope of a more satisfactory result in the future, we placed him under the influence of chloroform on the 15th of February, and very soon afterwards arrived at the conclusion that he was the subject of a stone. Believing this to be somewhat smaller and more friable than it really was, and anxious to economize as much as possible in his favour, I introduced a lithotrite, and endeavoured to grasp the stone. In this attempt, however, both myself and colleagues entirely failed, and subsequent examination proved that it was unsuited, by reason of its size and constitution, for lithotrity. No other alternative offering, I applied to the Deputy Surgeon-General of the circle for permission to perform the other operation, and this being readily granted, I cut the man in the usual way, in presence of my colleagues, on the 23d of February, and happily relieved him of the cause of all his troubles. The stone was found to be a very typical specimen of the mulberry variety. It weighed upwards of an ounce, and measured about 1½ inches in its longer and 1¼ in its shorter diameter. From the number of spiculæ with which its surface is studded, it resembles a prickly pear, and these would seem to be so contrived as to cause the greatest amount of suffering and distress. He recovered fairly from the shock of the operation, and some urine passed per urethram two days afterwards. It continued to dribble through the wound as well as through the penis up to the sixteenth day after operation, when the natural passage absorbed the greater part of it, and meanwhile the progress of the wound towards union was very satisfactory. He has been allowed wine and brandy in small quantities all along; these have been latterly replaced by porter, and he may now (23d March) be regarded as convalescent. I have lately seen him here, so much altered in point of appearance as to be scarcely recognisable as the same haggard individual I operated on in February last. He is to be invalided to England

and the change will, I hope, enable him to shake off any trace of his old ailment that may still linger behind.

Remarks.—This is a suggestive case, from whatever point we may choose to regard it; and if the rarity of such occurrences amongst soldiers may be allowed to form any measure of interest, it would on this score alone deserve notice, for it is the first of the kind I have met in a service of seventeen years, and one may spend a lifetime in the army without coming across a single similar instance. There is no doubt in my mind that this man has been the subject of a calculous formation from a very early period of his life. He himself expressly says that he has been suffering "from boyhood from a difficulty connected with the bladder; that he was not able to take part like other boys in outdoor sports, or pass his water without pain." He adds, that he was several times examined in a public hospital in Scotland, in the presence of students, and by a very eminent surgeon, the author of an elaborate dissertation on this very subject, as far back as 1860. He was again subjected to examination in several positions, by a departmental medical officer, at Colaba, near Bombay, in April 1872. Whether these gentlemen discovered a stone or not does not appear; anyhow, no action was taken in the matter at the time, and the absence of stricture then and now is proved by the passage by me of the large lithotrite that was supplied to me by Mr Blaise, of St James Street. How, then, does it happen that this stone—and mulberry calculi are notoriously products of slow growth—which gave such abundant proofs of its presence, remained so long undiscovered? I can only answer this question by supposing that the failure was due either to faulty or imperfect manipulation, or to the employment of media and means which were unsuited to the occasion. Among these the sound must ever hold the first place; and it accords with my experience that the instruments of this class usually supplied by the public are too large and unwieldy for the detection of a small stone, and that the smaller this weapon is in the body and the handle, and the shorter it is in the curve and the beak, the better and more available will it be for the patient and for the surgeon. This, too, is, I am glad to find, the opinion of Sir Henry Thompson (see *Lancet* for 20th December 1873); and I am equally pleased to find that that eminent surgeon deprecates the use of injections, or other similar preparatory treatment. I have acted on that principle for years, and it is now, I believe, the common mode of procedure in this country.

Sometimes failure follows from too much caution, and it has more than once happened to me to take hold of a sound, whose beak I found wagging in space, just under the arch of the pubis, and to find the object I was in search of on thrusting it farther ne. Then, again, too much is sometimes expected from it, and e who regulate their practice more by the statements of others than by the results of their own personal observation, will often find themselves wofully at fault if they expect to hear, in all cases, what they call a "ring like metal," or Erichsen, with more periphrasis, "a characteristic and distinct shock, ... a tolerably loud click, which can be detected by the bystanders, and frequently by the patient as well as by the surgeon." No doubt the auditory nerves of some persons are much more susceptible of such impressions than those of others, and the variations of sound have ever been a fertile source of difference even among the initiated. But this is no reason why we should always look for or require such manifestations as those indicated above, and I have often been obliged to content myself with a roughness or a collision, a touch or a graze, which, though it neither partook of the nature of a "ring," or even of a "click," was yet sufficiently characteristic of the presence of a stone to justify me in attempting its removal. Of course, when the stone is of large size, and fills a considerable portion of the bladder, there ought to be no difficulty in finding it. My remarks do not, however, contemplate any such condition. I am of opinion that "it is not less important to be capable of finding a stone when small, and determining its size, than it is to perform the operation afterwards;" and it is obvious that had such a discovery taken place at the proper time in this man's case, this stone might have been crushed, and its victim thereby saved the risk of a cutting operation, and spared, besides, many years of indescribable suffering.

ARTICLE IV.—*Enlargement of the Thyroid Body in Pregnancy.* By LAWSON TAIT, F.R.C.S. Ed. and Eng.; Surgeon to the Birmingham and Midland Hospital for Women, and to the Lying-in Charity; Consulting Surgeon to the West Bromwich Hospital, etc. etc.

(Read before the Obstetrical Society of Edinburgh, 10th March.)

I NEED not here discuss the many and very various interpretations of the physiological import of the thyroid body, for I have met with none which seems to me to be more than a guess. All that we know of certainty is that it is a structure of much greater importance in the economy of the female than in that of the male, being in this respect somewhat analogous to the mammary gland; for it is always larger, and its diseases are much more frequent, in women than in men. Thus the remarkable vascular hypertrophy which constitutes one of the phenomena of the exophthalmic goitre of Begbie, Graves, Basedow, and Trousseau is far more common among women than among men; so also are the cystic and fibrous forms of hypertrophy; whilst the simple adenoid increase in size is almost unknown among males. In women there seems to be a recurrent variety of this enlargement which is directly dependent on, or at least coincident with, certain epochs of their genetic functions, especially during pregnancy.

This puerperal enlargement of the thyroid body would further seem to me to be dependent upon some endemic cause; for on no other ground can I explain the vari us estimates given by authors as to its frequency, and the singular fact that, while resident in Wakefield, I saw numerous cases of it, a selection of which will be afterwards detailed, whilst in Birmingham I have seen only one case out of a very large *clientèle* of hospital patients, and in that case the patient had not resided long in this town.

Natalis Guillot is, as far as I know, the first author who has drawn attention to a specific enlargement of the thyroid during pregnancy. His paper is in the *Archives de Médecine*, tom. xvi., and he describes the post-mortem of two fatal cases in which the goitre was found to be constituted by a hypertrophy of the fibrous and glandular elements. Dr Tanner narrates a fatal case in his work on "Diseases of Pregnancy," but that seems to have been of the vascular variety, associated with exophthalmos, and therefore not such as I have to allude to. Simpson notes enlargement of the thyroid in pregnancy as an "occasional and rare" occurrence; whilst Holmes Coote, in his paper on "Diseases of the Thyroid," in Holmes's "System of Surgery," says that it has been not uncommonly noticed.

In a recent paper entitled "Etudes sur les Maladies Chroniques d'Origine Puerperale," by Dr Auguste Ollivier, in the *Arch. Gen. de Médecine*, January 1873, this peculiar condition is very carefully described; and as none of that author's conclusions tally very closely with my own, I may here briefly summarize them. He finds that puerperal goitre may assume various distinct forms, of which the first is the *goitre subaigu et passager*. This is developed slowly, and only reaches any very considerable size in exceptional instances. It causes no pain, and generally the patient goes to her full time without experiencing any disturbance of respiration. There is no such pulsation as marks the vascular goitre, or that which accompanies exophthalmos. After delivery, it diminishes gradually in volume, and after a few weeks or months, no trace of it is to be found. It usually appears about the third or fourth month of pregnancy.

In Dr Ollivier's second variety, the goitre is developed very rapidly, and gives rise to accidents by suffocation. I have not seen a case of this kind associated with pregnancy, and I suspect that in it the enlargement belongs to the vascular elements of the gland; for vascular bronchocele of rapid or even sudden growth, and with or without exophthalmos, is not at all rare, independent of pregnancy. We can readily understand how such an occurrence would be much more serious in a pregnant woman.

In the third variety of puerperal goitre described by Dr Ollivier, enlargement proceeds slowly during the course of pregnancy, remains stationary after delivery, or may undergo slight increase in size during each succeeding gestation. Sometimes it succeeds a goitre of rapid growth which has been arrested in its development. In some cases the goitre does not appear, or is only recognised some time after a pregnancy. It progresses slowly and insidiously, and without any appreciable cause. This form is far from rare, and its real origin may not be recognised *faute de prevenir*. Instead, however, of remaining stationary, the chronic puerperal goitre is sometimes seen, at a period more or less remote from the pregnancy during which it appeared, to increase rapidly in size so as even to threaten suffocation. After a time it may also be subject to inflammation and suppuration.

For my own part, I cannot see sufficient distinction between Dr Ollivier's first and third varieties to enable me to consider them as other than different phases of the same condition.

My first experience of puerperal goitre was in the year 1863, when I was acting as assistant to my much esteemed friend Dr David M'Farlane of Drymen. On 11th December I was called to see Mrs M., æt. 32, who was in labour of her fifth child. I noticed that she had a large goitre, and, on asking its history, she told me that it appeared in her first pregnancy about the fifth month, gradually increased in size, and completely disappeared soon after labour. It reappeared during her second pregnancy, and similarly grew and disappeared. So in her third, but it did not disappear entirely after her labour. It grew rapidly soon after she became pregnant for the fourth time, became larger than ever, and diminished only slightly after her labour. She first suspected her fifth pregnancy from the increase of the bronchocele. She had had pretty smart flooding after her fourth labour, and I anticipated that the same complication would occur in the one for which I was attending her, and I was not wrong. Labour was natural and easy, but the uterus was very flaccid, and I had much trouble in keeping it contracted. As it was, she lost an unusual though not an excessive amount of blood. I saw her occasionally for two or three months after her labour, but the goitre did not perceptibly diminish, and she remained somewhat anæmic.

This woman was a native of Carluke, and had been married there. She told me that such enlargements were not uncommon in the necks of the women in that neighbourhood, but that she did not remember having seen one in the neck of a man.

All the other cases which I have seen, practically presenting the same features as the above, number about twenty, and all, with one other exception, were natives of and residents in the vale of Calder, of which the centre is Wakefield. They came under my care there as hospital patients, and the majority of them applied for ailments which had either no relation to the goitre, or only an indirect one. Out of some thousands of patients I never saw a man with a goitrous enlargement in that district, nor one in a woman who had not borne children. The town and district are physically well situated, the ground being gently undulating, with a deep subsoil of loose drift lying on very porous sandstone, and the watershed is almost exclusively millstone grit. The drinking water is mostly spring or surface well-water, except in the town, where the supply is taken from the river, filtered, and it is marked by a rather abundant quantity of lime, so that stone is rather a prevalent disease.

The trade is chiefly wool-spinning, and the people are of a healthy Saxon type. I cannot suggest any feature which would explain the endemic occurrence of this peculiar disease. In the majority of the cases there was a marked tendency to uterine hæmorrhage, which, when the story of the case could be intelligibly given, was generally found to be of subsequent date to the appearance of the goitre. Those women in whom the goitre was of old standing were always anæmic, and an almost uniform large and hard condition of the uterus was found, the lips being thick and the cervix open, and the menstrual periods were too frequent and very profuse. The salts of potash, especially the bromide, had a uniformly good result in arresting the excessive flow, whilst iron always increased it without benefiting the patients in any way; but no kind of treatment was found of any use in diminishing the size of the goitre. It would be tedious to narrate the therapeutics to which each case was subjected, and I therefore give the above summary of the results.

I have preserved notes of the following cases:—

Case I.—S. R., aged 43, has had eleven children. When pregnant for the tenth time she noticed a goitre, which appeared during the later months of pregnancy and disappeared rapidly after delivery. It reappeared during her eleventh pregnancy, and diminished very much in size after delivery, but did not altogether disappear. In 1867, she had a miscarriage at the fourth month, by which time the goitre had much increased from the size at which it had rested in the interval between her eleventh and twelfth pregnancies. Nine months after, she again miscarried at the third month, by which time the bronchocele had again increased, and since then it has not (till March 1868) diminished perceptibly in size. Since the last miscarriage she has had two severe attacks of flooding at the monthly periods. The goitre is of large size and three-lobed, showing distinctly the exaggerated outlines of the anatomical divisions of the gland. The floodings were completely arrested by a liberal administration of bromide of potash.

Case II.—The woman in the case detailed above brought me her sister, because she also had a goitre. She was 36 years old and had had two children. In her the goitre made its appearance for the first time during the later months of her second pregnancy, vas only beginning to disappear, some months after her confinement, when I saw her, in August 1868. It was stated by both sisters that in this case the goitre had been at least three times larger during the pregnancy than it was when I saw it. She menstruated too often and very profusely.

CASE III.—Mrs W., aged 36, presented herself at the hospital on account of symptoms of well-marked spanæmia, associated with profuse menorrhagia. She had a large three-lobed goitre, of which she gave the history that it appeared first as a small lump when she was about three months gone in her first pregnancy, being then twenty-three years of age. It disappeared after delivery, but reappeared with increasing size in each succeeding pregnancy. It never wholly disappeared after the first confinement, but after every other it diminished considerably in size. When I first saw her the neck measured eighteen inches and a half in greatest circumference, and there were occasional attacks of dyspnœa, though this latter condition may have arisen from the spanæmia. She had seven living children, with one miscarriage between the sixth and seventh, and three miscarriages since the seventh child. She stated that at each of the four miscarriages, and at the sixth and seventh confinements, she had lost a great quantity of blood, and that this had not been the case in the confinements previous to the sixth. I attended her for the last miscarriage, and the hæmorrhage then was quite alarming. She had been made aware of the last three conceptions before the completion of the month by the rapid increase of the goitre, and this matter of detail she gave me without any leading question on my part, and as a point on which she was positive. The increase on each occasion was so great as to make an uncomfortable difference to her breathing. The menorrhagia was completely subdued by full doses (thirty grains) of the bromide of potash, though no difference was noticeable on the tumour. There was no appearance or history of exophthalmos or vascular disturbance.

In August 1868, I made an experimental trial of the subcutaneous injection over the goitre of ergotin in doses of ⅓d of a grain, but the constitutional disturbance, indicated chiefly by fainting and sickness, was so great after each injection that it was only repeated thrice. It made no difference to the goitre.

CASE IV.—H. M., æt. 37, had had three children, and in each pregnancy a goitre had appeared at about the fourth month, and in each successive pregnancy it was larger than it had been during the preceding one. It always had rapidly disappeared after confinement. During the interval between the second and third pregnancies, and ever since the last, she has had profuse menstruation, till her condition at the time of my first seeing her was one of serious spanæmia. Her last pregnancy had occurred three years previously, and the goitre which remained from it was well marked. She was ordered twenty-grain doses of the chlorate of potash, to be taken with every meal, and this had the effect in two months of completely subduing the excessive menstruation, and in her opinion, though not in my own, of reducing the size of the goitre.

CASE V.—Mrs G., aged 38, seen first on 5th March 1869, with a large solid goitre. She had noticed it first when about five months pregnant of her first child, she being then twenty years of age. It increased in size until the child was born, and then rapidly disappeared. It reappeared during the second pregnancy at about the same period as in the first, but it grew to a much larger size. It diminished very much after the second confinement, but did not disappear entirely. She has since had eleven children, and the commencement of increase in size of the goitre has always occurred at about the same time in each pregnancy; and at every increase it has become larger than it was on the occasion previous. She gives no history of post-partum hæmorrhage or of menorrhagia. She had no very marked anæmia, though she looked worn down and prematurely old, and she applied for treatment of an ailment which had no relation to the goitre.

CASE VI.—S. J., aged 60, had her first baby at twenty-two, and she remembers noticing the growth of a lump in her neck during the last two or three months of her pregnancy. It disappeared soon after her confinement. At twenty-five she had her second child, and the lump reappeared about the same time, or perhaps rather earlier in the pregnancy, but it did not entirely disappear after her confinement. She has had thirteen subsequent pregnancies, and in every one the goitre began to increase in size about the fifth month, and diminished somewhat after delivery. She suffered for many years from profuse menstruation, dating, she thinks, from the birth of her seventh child, and she had the change of life at fifty-two. She was extremely anæmic, and was only a short time under notice.

CASE VII.—Betsy L., aged 33, was married at sixteen, and had her first baby when she was nineteen. She distinctly remembers that during the sixth month of that pregnancy a lump began to grow on the front of her neck, and that it went on increasing in size until her confinement, after which it disappeared entirely in a few days. It reappeared at the same time in her second pregnancy, and in four succeeding pregnancies. It was bigger each time, but always completely disappeared soon after confinement. On her seventh pregnancy it grew to a very much larger size than on any previous occasion. This pregnancy proved to be twins, confinement having taken place eight months previous to my having first seen her. The goitre had not wholly disappeared, but was large enough to attract my attention and lead me to inquire into its history. At all her confinements, she had had serious hæmorrhage; and ever since her first confinement she has menstruated every three weeks, save during gestation and lactation, the period always lasting eight or nine days, and the loss being very profuse. She was a stout handsome brunette; and though there was no reason to doubt her story, her appearance certainly did not support it, for she was not in the least spanæmic-looking.

CASE VIII.—Emily S., aged 33, has been married fourteen years, and has had six pregnancies. In the course of every one, the thyroid has enlarged during the last three or four months, and has always nearly disappeared after confinement. When I first saw her, 25th March 1870, she had a well-marked goitre, but she stated that it was only about half the size it had been during the ultimate month of her last pregnancy, December 1869. She was very anæmic, and suffered very much from dyspeptic symptoms.

CASE IX.—Emma D., aged 38, has been married twenty years; has been confined eight times at the full period, and has had eight miscarriages. When I saw her in October 1869, she had a well-marked goitre, which she stated had been in existence since puberty, though it had been of small size previous to her first pregnancy, during which it increased so rapidly in size that it interfered seriously with her breathing. After her confinement it rapidly resumed its original size. Both before and after marriage her menstrual periods were always profuse and prolonged. She also always lost an excessive amount of blood at her confinements. She stated that she thought the goitre had increased in size with each pregnancy, but on this point she was not very clear. She had no vascular or nervous symptoms.

CASE X.—Eliza S., aged 43, was married when twenty, and had her first child within the year. A goitrous enlargement appeared during her fifth pregnancy, between the fifth and sixth months. She had six subsequent pregnancies; and though the goitre entirely disappeared after the fifth and sixth confinements, it has increased in size with all those subsequent, and in January 1870 it was very large and hard, but gave her very little inconvenience. Since the existence of the goitre she has had serious floodings at her labours, and her menstruation has been very profuse.

CASE XI.—Harriet H., aged 44, was married at twenty-four, having had a child two years previous. Her third pregnancy occurred when she was twenty-eight, and an enlargement in the neck was noticed about the fifth month. It disappeared after her confinement, but grew again in her fourth pregnancy, but did not again disappear. It increased in size with every succeeding pregnancy, and she was under the impression that its growth was specially influenced by a copious bleeding, to which she was subjected with a view of diminishing it. She always has had flooding at her confinements, and has always menstruated profusely. I only saw this patient once, but her goitre was the largest I have ever seen, the greatest measurement round the neck being twenty-four inches.

Case XII.—Mary Ann H., aged 38, an out-patient at the Birmingham Hospital for Women, had no indications of goitrous enlargement before marriage. It appeared during the first pregnancy, and did not altogether disappear after the confinement. It has gradually increased in size with each succeeding pregnancy, diminishing somewhat after each delivery. She is very anæmic, and has suffered for many years from profuse menstruation, but does not know whether this was or was not antecedent to the appearance of the goitre. She was born in Worcester, and has lived in Hereford, Gloucester, Ludlow, and lately in Birmingham.

In many features these cases are perfectly uniform, whilst in others they present some striking peculiarities, of which a little may be said. Thus in every case but one (IX.), the goitre appeared first during a pregnancy, and even in the exception it is probable that the uniformity is not broken, for in that case only was the first growth of the goitre very acute. Probably the tendency to this peculiar condition existed before it was so excited by pregnancy. In only one case (V.), the tendency to hæmorrhage was not present; and of those in which the history was given decidedly, the majority state that the appearance of the goitre preceded the metrorrhagia. There seems also a marked tendency to miscarriage.

I have not been fortunate enough to have a post-mortem examination of the thyroid of a case of which I have known the history; but in 1869, I made a medico-legal examination in Wakefield of the body of a woman who was six months pregnant, who had had several children, and who had a well-marked goitre. I could, however, obtain no history of the goitre, but I have little doubt it belongs to the variety met with in the cases I have detailed; and in the absence of more precise evidence of the structure of these goitres, I may describe this one.

On making the usual incision and reflecting the integuments off the gland, all its parts were seen to be uniformly enlarged—that is, both lobes and the isthmus—and the lobes wrapped round the posterior borders of the trachea. It was increased, roughly, to five times its normal size, and the right lobe was the larger of the two. On section, the structure was everywhere normal, no cysts, sinuses, or dilated vessels being seen anywhere. Several small pieces were removed for microscopic examination. They were placed in a bottle containing equal parts of a two per cent. solution of chromic acid and methylated spirit. The bottle was labelled, but unfortunately misplaced, so that it was not till November 1874 that an examination of the fragments was made. By that time they were very hard, and cut easily, but did not seem in the least brittle or altered in structure.

A section stained with carmine, and examined by a low power, showed that the fibrous elements generally were exaggerated, and at one spot, in the neighbourhood of a vessel (Fig. I.), this was so marked as to be visible to the naked eye. As this vessel was the only one in the section, it was impossible to determine whether it was an artery or a vein, but its appearance was more that of the latter. The fibrous bands running between the alveoli seemed to enter into the coat of the vessel so intimately, that I was at first led to suspect that I had to deal with bundles of unstriped muscular fibre, and the regularly fasciculated appearance presented here and there by the bands favoured this suspicion. No examination by reagents, however, displayed the rod-shaped nuclei which characterize unstriped muscular fibre.[1]

In this same section were seen a number of alveoli, of irregular outline and distribution, which were filled with fat and epithelial debris. These were evidently old follicles, the contents of which had undergone fatty degeneration and were being removed; and I think that the fibrous hypertrophy in the neighbourhood of the vessel must be due only to the coalescence of the fibrous bands consequent on the removal of the follicles.

In another section, stained with hæmatoxylin, and examined with a higher power, a different and indeed quite opposite state of matters was displayed (Fig. II.). Three follicles in the field are shown as having attained what is probably their mature size, and in one there were three brightly refracting points of a yellowish colour, which showed that the process of fatty decay had begun, but still the epithelial elements were perfect. The majority of the follicles were, however, of small size, and the largeness of the cells with which they were stuffed showed that they were just starting into life. A few follicles were occupied by light-brown colloid matter, which, from the coincident absence of the epithelial lining, seemed to indicate an arrest of development of the follicle.

This case seems, therefore, at least to illustrate the follicular hypertrophy of the thyroid, described by Rokitansky, an increase of size due merely to a multiplication of its normal elements—a true hypertrophy, due probably to an increase of the amount of work the gland has to do. To my view, it also explains the process of the periodic enlargement of the thyroid, which I have described in the cases detailed above.

Let us suppose that, during an early pregnancy, the thyroid increases in size by a rapid and considerable increase in number of its follicles, due to some physiological necessity, the nature of which we do not know, but which is in existence only during pregnancy. With the growth of the follicles the inter-alveolar fibrous tissue must also increase; but when the follicles die and are absorbed, that fibrous increase remains entirely or in great part, its removal being no such easy matter as is the removal of epithelial cells and their products. During the second pregnancy in which the goitre appears, the new follicular growth would seem to have a fresh site, for apparently it is not reproduced in the midst of the fibrous and fatty remains of the old follicles; and here again it would leave behind it another cirrhotic addition to the permanent bulk of the gland, after the end of the pregnancy and the removal of the temporary increase of the follicles, which I have supposed to exist. This repeated time after time would explain the facts common to all the cases I have met with, that after the goitre has appeared it becomes larger during each successive pregnancy; and that on each successive diminution after delivery, it is found to have received an accession to its permanent bulk.

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[1] Dr Matthews Duncan kindly examined a section, and substantiated this observation.

As this disease seems to have been, by circumstances, removed from my field of observation, I have failed to bring to bear on it more extensive and painstaking investigation; and, perhaps, the conclusions I have offered may seem to be based on insufficient experience. But I submit them mostly with hope of drawing the attention of others more favourably situated with regard to this curious condition, a careful examination of which may yet afford a clue to the physiological significance of the thyroid gland.

ARTICLE V.—*Notes on "Changed Aloin" and the Resin of Aloes.* By WILLIAM CRAIG, M.D., C.M., F.R.S.E., F.R.C.S.E., Lecturer on Materia Medica, etc., Edinburgh School of Medicine. Part I.

ALOES, "the inspissated juice of the leaves of various species of aloe," has been used in medicine for upwards of 2000 years. It was not, however, till 1851 that the Messrs Smith of this city discovered in aloes a crystalline principle, to which they gave the name of aloin, because it was supposed by them to be the active principle of the drug. After this important discovery of aloin, much difference of opinion existed regarding its physiological action. So late as 1870, we find Dr Tilden of London maintaining that "the active constituent of aloes is still unknown," "that the purgative property is not due to aloin," and suggesting that the purgative power is due to the resin of aloes. Notwithstanding these statements, it has been clearly demonstrated by various experimenters, and confirmed by clinical observation, that aloin is an active aperient, and is sufficient to account for the purgative property of aloes.

It was no part of my purpose, in the following experiments, to investigate the properties of aloin, about whose action there ought to be no reasonable doubt, but to investigate the properties of "changed aloin," and to determine the action of the so-called resin of aloes.

In 1872, when preparing a specimen of aloin for the Vienna

Exhibition, the Messrs Smith took four ounces of aloin and dissolved it, with a view to recrystallization, in order to obtain very pure crystals of aloin. From this solution two ounces of very beautiful crystals were obtained. The residue was set aside for eight months, when it was found that a substance had formed, easily reducible to a fine powder. This substance was uncrystallized, and in appearance not altogether unlike the resin of aloes. To this substance I have given the name *changed aloin.* There can be no doubt about its being aloin, which had undergone some chemical change by exposure to the air, probably a species of oxidation. They were naturally anxious to ascertain if this aloin which had undergone some chemical change still possessed its physiological action. They applied to me to investigate this subject, which I accordingly did, and found it possessed of all the activity of aloin itself. My experiments were performed on a healthy young rabbit. I had a cage so constructed that I could easily weigh the fæces and measure the urine passed in twenty-four hours. For six days I fed the rabbit daily, at a fixed hour, on ten ounces of carrots, and each day ascertained carefully the amount of fæces and urine passed. During these six days no changed aloin was administered. The results are given in the following Table:—

TABLE I.

No Injection.

1st day,	urine	56	fluid drachms,	fæces	0.
2d	„	74	„	„	0.
3d	„	68	„	„	0.
4th	„	53	„	„	0.
5th	„	60	„	„	traces.
6th	„	64	„	„	„
Average,	„	62·5	„	„	„

During the next six days, I injected daily, at a fixed hour, one grain of this changed aloin in solution. The injections were made subcutaneously on the sides of the animal. The animal was fed on the same diet, and at the same hour, as during the previous six days. The amount of urine and fæces was daily ascertained, and the results are given in the following Table:—

TABLE II.

One Grain of Changed Aloin Injected Daily.

1st day,	urine	48	fluid drachms,	fæces	59	grains.
2d	„	60	„	„	121	„
3d	„	68	„	„	184	„
4th	„	60	„	„	38	„
5th	„	64	„	„	10	„
6th	„	64	„	„	8	„
Average,	„	60·7	„	„	70	„

On comparing carefully these two tables, it will be seen that the urine remained much the same during the whole twelve days. The average during the first six days was 62·5 fluid drachms, and during the second six days 60·7 fluid drachms. The urine was slightly diminished, but the difference was very small, and quite within the range of variability. It was very different with the fæces. During the first six days there were no fæces, or only traces on the fifth and sixth days. During the second six days the average amount of fæces was 70 grains daily. The first day after the injection of the changed aloin, there were 59 grains of fæces, the second day 121 grains, and the third day 184 grains. The amount gradually diminished during the next three days. For three days there was a gradual increase, and, afterwards, a gradual decrease for three days. This state of matters is easily accounted for. Having once freely emptied the bowels by means of changed aloin, we could not expect the same amount of fæces to be continued whilst there was no increase of food. Nevertheless, the results were so marked and followed so soon after the injection, that there is no resisting the conclusion that this changed aloin acted as an aperient, and possessed all the activity of crystallized aloin.

Being anxious to ascertain the action of this "changed aloin" on man, I had it made into pills with the extract of gentian, each pill containing from one to two grains of changed aloin. These pills I administered to various individuals and in very different circumstances, and in all cases found them a mild but certain aperient. I administered these pills to several persons of sedentary habits, and succeeded in removing the constipation so common in such circumstances. I found that one or two grains daily of changed aloin were sufficient to accomplish that object. I also gave these pills to pregnant females shortly before confinement, and in several cases administered them to females a few days after they had been delivered, and in both cases found them to answer exceedingly well. In no case was there any griping, nor did any bad result follow. I thus found that clinical observation fully confirmed the conclusion drawn from experiments on the lower animals, that this aloin, which had undergone considerable change by exposure to the air, still possessed all the activity of the active principle of aloes. These experiments are interesting and of considerable value, as proving that aloin, which undoubtedly is the active principle of aloes, may undergo considerable chemical change without losing its physiological action. In the second part of this paper, I shall have to show that they have an important bearing on the physiological action of the so-called *resin* of aloes.

ARTICLE VI.—*Hæmatinuria.* By J. W. BEGBIE, M.D.

(*Read before the Medico-Chirurgical Society of Edinburgh, 7th April* 1875.)

THIS disease, in which the passage of the colouring matter of the blood, hæmatin, with the urine takes place, has of late years been described by Dr George Harley,[1] Dr Dickinson,[2] Dr Greenhow,[3] and Sir William Gull,[4] while Dr Roberts of Manchester, in his work on Urinary and Renal diseases, besides detailing interesting examples of the disorder which have fallen under his own observation, has given an excellent summary of the whole subject of hæmatinuria.[5]

Antecedent to the publication by Dr George Harley, in the Transactions of the Medico-Chirurgical Society, of two cases of this remarkable malady, the circumstance of hæmatin or hæmato-globuline existing in the urine without the association of blood-discs had been noticed by different pathologists. Thus, Neubauer and Vogel remark—"The urine sometimes presents a bloody or reddish-brown, or brownish-black, or even an inky coloration, and yet, under the most careful microscopic observation, blood-corpuscles cannot be detected in it." Urine of this kind, these authors go on to observe, "is occasionally met with in diseases, which are associated with, what is called a dissolved state of the blood—in scurvy, in putrid and typhus fevers, in malignant remittent fever, after the inhalation of arseniuretted hydrogen."[6]

Dr Parkes observes—"Hæmatine appears in the urine in two states, either in the blood-corpuscles or separate from these; . . . in the second, the pigment is completely dissolved in the urine, to which it gives a more or less brown or black colour. Dissolved hæmatine," Dr Parkes adds, "appears to indicate not local disease and rupture of vessels, but a special affection of the blood, either general or local, produced by some septic or profound cachectic diseases. In bad typhus, malignant variola, pernicious remittents, etc., in scurvy, and sometimes in morbus Brightii, the urine may be very dark from hæmatine; and the very dark urines, if bile and vegetable pigments be absent, almost always indicate its presence."[7]

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[1] On Intermittent Hæmaturia; with Remarks upon its Pathology and Treatment. Medico-Chirurgical Transactions. Second series, volume xxx. page 161.

[2] Notes of Four Cases of Intermittent Hæmaturia. Medico-Chirurgical Transactions. Second series, vol. xxx. page 175.

[3] Case of Intermittent or Paroxysmal Hæmaturia. Transactions of the Clinical Society of London. Vol. i. page 40.

[4] A case of Intermittent Hæmatinuria, with Remarks. Guy's Hospital Reports. Third series, vol. xii. page 381.

[5] Second edition, page 139.

[6] A Guide to the Qualitative and Quantitative Analysis of the Urine. Sydenham Society's translation, page 310.

[7] The Composition of the Urine in Health and Disease, and under the Action of Remedies, page 183.

Dr Thudichum describes hæmatin or hæmato-globuline as dissolved blood-corpuscles, "for," he observes, "it is in fact a mixture of the albuminous fluid filling the corpuscles—globuline, and of the colouring matter, which may be obtained in crystals under certain conditions—hæmato-crystalline." The distinction drawn by Dr Thudichum between dissolved blood and dissolved blood-corpuscles, hæmato-globuline, is without doubt of importance. He was the earliest to direct attention to this particular, and to point out that "urine may contain albumen and hæmato-globuline at the same time, yet they must be present in the same proportions as in the blood, before we can say that they are due to hæmorrhage and consequent solution of the blood."[1]

The clinical history of hæmatinuria may be said to date from an earlier period than that of the publications referred to, inasmuch as cases of a very similar nature had already been described or alluded to by Dr Prout,[2] Dr Elliotson,[3] and Sir Thomas Watson,[4] also by Mons. Rayer, and Mons. Gergères of Bordeaux.[5] More recently Dr Pavy,[6] Dr Alfred Wiltshire,[7] and Dr Murchison,[8] have contributed to the illustration of this disorder.

In the accounts of hæmatinuria which have been given by the different authors to whom I have referred, there exists a very close resemblance. The disorder is essentially intermittent in character, or, as it has been styled, paroxysmal. The attack is preceded by a shivering, or at least a notable feeling of cold, and after the passage of the dark-coloured urine, any general disturbance of the system, and lumbar uneasiness or pain, if such have existed, decline. There is unquestionably such movement of the nervous system in many of the cases as to suggest the relationship of hæmatinuria and ague. Of twenty cases collected by Dr Roberts, "four had at one time or another suffered from undoubted ague, but in the remainder no evidence or suspicion of ague or malarial poison existed."[9] Sir Thomas Watson, in his Lectures, makes special reference to a connexion subsisting between urinary hæmorrhage and ague. Dr Elliotson, in the clinical lecture already mentioned, gives the following very interesting case:—"It was that of a man with a diseased heart and symptoms of ague; he was admitted into hospital on the 24th November. He was one of those unfortunate persons who were sent by a very wise Government to Walcheren, where so many thousands of our countrymen lost their lives for no purpose whatever. He had this violent fever of the place, and from that time was never perfectly well. He is forty years of age; he looked sallow and of a dirty pale hue, and on that account I asked him if he had had ague; to which he replied that he had had the fever at Flushing. Now this man was labouring under frequent chills, but had not regular paroxysms of ague. You will find it a common circumstance for ague not to be perfect in all its stages, but for various degrees of shaking to take place from time to time. That was the case with this man. The singular circumstance, however, in this man's disease was, that when his paroxysms came on he discharged bloody urine. . . . Now, in this man, the kidneys discharged blood, at least there was blood in the urine; at first pure blood, and afterwards less and less, and this he said was invariably the case—hæmaturia every time the cold fit came on. This circumstance, however, made no difference in the treatment, and I gave him sulphate of quinine. I mentioned, however, that he had had a disease of the heart. There was a great impulse of the left ventricle; but this was only a recent occurrence, and the cause of it I do not know. There was also a strong, full, sharp, pulse, and on that account I bled him to a pint, put him on low diet, and kept his bowels open every day. He was bled on the 25th to another pint, on account of the violent action. The aguish symptoms were now quite certain, for I had observed them myself, and I therefore gave him 5 grains of sulphate of quinine three times a day. He was bled again on the 6th of December to 16 ounces. He bore it well, and, in fact, was all the better for it. He then took 10 grains of sulphate of quinine three times a day till he became perfectly well, so far as his aguish symptoms were concerned. He lost the rigors, he lost the cold fit, and lost the bloody urine. The bloody urine was intermittent like the rigors; that is an interesting circumstance. I never met with an instance of a similar description. There can be no doubt of its truth, because the man showed his urine, and the blood was abundant in it. He was presently quite well, so far as this was concerned, and the symptoms arising from hypertrophy of the heart were much diminished. Having had aguish fever, however, in the severe form which he suffered, whenever the east wind blows, or he is exposed to cold and wet, or commits any errors in diet, or is guilty of any debauchery, he will be liable to a return of the disease."

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[1] A Treatise on the Pathology of the Urine, page 235.

[2] The Nature and Treatment of Stomach and Renal Diseases. 5th edition, page 414.

[3] Clinical Lectures. Diseases of the Heart united with Ague. *Lancet*, vol. i. 1831, 1832, page 500.

[4] Lectures on the Principles and Practice of Physic. 4th edition, vol. ii. page 726.

[5] Traité des Maladies des Reins. Tome troisième, page 370.

[6] Transactions of the Pathological Society of London. Vol. xviii. page 157.

[7] *Eodem loco.* Vol. xvi. page 183.

[8] *Eodem loco.* Vol. xviii. page 180.

[9] *Op. cit.*, page 141.

I think it worth while to give Dr Elliotson's concluding remarks on the case, which have a special reference to its treatment. "You have seen several times, in the course of the winter, that when bloodletting is indicated, the indication to give sulphate of quinine does not interfere with the depletion. This man was bled on account of the great impulse of the heart, three times to a pint, and the symptoms for which the bleeding was instituted were not aggravated by the quinine, nor, on the other hand, was the cure of the aguish symptoms by the quinine at all impeded by the bleeding. The diseased heart and bloody urine appeared to have no connexion with the other, but the bloody urine depended on the ague. Although the man considered himself quite well, his ague and hæmaturia having been cured, and the impulse of the heart having been considerably diminished, yet he will, of course, through the Walcheren expedition, be a shattered man as long as he lives."

M. Rayer, in treating of renal hæmorrhages, notices, under his third group—"Hémorrhagies Rénales Essentielles"—the case of Dr Elliotson just quoted, and likewise a very interesting one recorded by M. Gergères, in which the relation of aguish symptoms and hæmaturia is very clearly exhibited.[1] Rayer also refers to a similar instance previously published by a Dr Stewart.[2] It is, of course, conjectural that these were instances of hæmatinuria. I agree, however, with Dr Harley in thinking it[3] highly probable that the case recorded by Dr Elliotson is the exact counterpart of those he has described.

Paroxysmal hæmatinuria appears to be almost limited to the male sex; only one of the twenty cases collected by Dr Roberts occurred in a female. In the same series of cases, the age of the patients at the commencement of the disease ranged from two to forty-eight years. Two cases were under twenty, seven between twenty and thirty, six between thirty and forty, two between forty and fifty, while in three the date of invasion is not specified.

Whether connected with malaria or not, there can be no doubt as to the important influence of cold, more especially when associated with damp, in exciting the paroxysm of hæmatinuria. To this circumstance all observers of the disease have borne testimony. It is well illustrated in the two cases which have fallen under my notice, and to which I shall now call attention, leaving a few remarks on the pathology and treatment of this obscure disease for the close of my communication.

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[1] "Un cas analogue est rapporté par le Docteur Gergères:—Un jeune homme, capitaine de navire, jouissant habituellement d'une bonne santé, fut pris pendant deux heures de frissons très vifs, après lesquels se développa une forte chaleur: pendant cette période, le malade eut besoin d'uriner; mais, au lieu d'urine, il rendit par l'urèthre une grande quantité de sang. Quelques heures après, une sueur s'établit, et le malade se crut guéri. Le lendemain, à la même heure, retour des mêmes accidens fébriles, et de l'évacuation du sang. M. Gergères prescrit un traitement émollient tant interne qu'externe. Les symptômes cédèrent encore à la même heure que dans le premier accès. Le troisième jour, les mêmes phénomènes se reproduisirent encore avec plus de violence. Dès-lors on s'empressa, vers la fin de l'acces, d'administrer le sulfate de quinine à la dose de vingt-cinq grains; ce moyen mit fin à tous les accidens, et en empêcha le retour."

[2] "Antérieurement Stewart avait publié l'observation d'une hematurie periodique, traitée sans succès, depuis huit mois, par la diète et le régime antiphlogistique, et qui guérit dans l'espace de trois mois par le quinquiina et les toniques. Le sixième mois de ce traitement, le malade était completement rétabli."

[3] *Op. cit.*, page 173.

A. H., æt. 25, a clerk; in height 5 feet 4½ inches; of weight 8 stone 7 lbs.; an orphan. Has two brothers, both of whom are healthy. First seen on the 11th December 1874, and again on the 18th of the same month, when he informed me that, for fully three months, he has not ceased to pass dark urine daily after midday. He had good health till September 1873, when he, for the first time, noticed the dark colour of the urine. The dark appearance of the urine was not at that time persistent; it occurred on an average about once a week for some weeks. After a few weeks, the urine resumed its normal appearance, and this continued till the autumn of 1874. He had overtaxed his strength by walking to Queensferry, a distance of nine miles, to visit the Channel Fleet, and had also been exposed to cold and damp. After this the dark urine reappeared. He has never received any blow or injury over the loins, nor has he at any time experienced pain in the lumbar region.[1] The urine has on no occasion been dark before 11 A.M. His usual breakfast has consisted of a cup of tea with sugar and cream or milk, or of porridge with milk, with a small piece of ham, and a little bread or toast. On Sundays, when keeping in the house, during the whole day, the dark urine does not appear. Before the dark urine is passed, he invariably feels general discomfort, and a sensation of coldness over the whole frame, but this has never amounted to a shivering or rigor. He is very pale, with a distinct icteric tint of the conjunctivæ. A well-marked anæmic murmur is audible over the base of the heart, accompanying the systolic sound, and a loud bruit de diable exists in the neck. There are no scorbutic marks on the surface of the body, no petechial spots, and no other form of hæmorrhage has at any time occurred. The patient's blood, when viewed under the microscope, shows no excess of colourless corpuscles; there is, however, manifest diminution of the red globules. The urine, after reposing for a brief time in a conical glass vessel, presents the following characters:—is of dark colour resembling port wine, possessing a fleshy odour, of acid reaction, having a density of 1022, highly coagulable on the application of heat and addition of nitric acid, a dirty-brown precipitate being thrown down by the former, and by the latter a deep chocolate-coloured mass.

A very considerable brownish sediment falls to the bottom of the glass, and this, when viewed under the microscope, is found to consist of reddish or brownish-red granular matter, with numerous granular casts of the tubuli uriniferi.

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[1] I make this observation more particularly in connexion with the following interesting statement of Sir William Gull:—"There is reason for thinking that a blow or injury to the loins may give rise to this complaint—intermittent hæmatinuria. Thus, a young lady, in getting into a railway carriage, fell and hurt her back. Soon afterwards she passed dark bloody-looking water. I carefully examined the secretion by the aid of the microscope, but found in it no blood-corpuscles, and only the granular pigment matter of disintegrated blood-corpuscles."—*Op. cit.*, page 390.

Dr Affleck, who kindly undertook a careful chemical examination of the urine of this patient, both when abnormally coloured and when free from blood pigment, has favoured me with the following report:—"The investigation bore reference more especially to the amount of urea, and to the presence of the bile acids. 1st, As to the urea. I found, after repeated experiments, in which a marked uniformity of result was obtained, that the urine, without hæmato-globuline, contained nearly one-fourth more urea than that with it, viz., 92 and 93 grains in the former, and 71 and 81 grains in the latter, per 12 ounces of urine respectively. 2d, As to the bile acids. After a long and careful search, I failed to find any distinct evidence of their presence in either specimen of urine. The plan pursued was that laid down by Neubauer and Vogel in their work. Ample time was given, and full justice done to all parts of the process, and I believe, had bile acids been present in any appreciable quantity, they would have been detected." Dr Affleck adds, "I ought to mention, that in both investigations I enjoyed the able assistance of Mr A. D. Murray. Although the result is largely negative, yet even this may not be without its use."

Respecting the elimination of urea, Dr George Harley had observed, that during the attack of intermittent hæmaturia in the cases recorded by him, the passage of urea was excessive, and that copious deposition of urates also took place.[1] Dr Dickinson and Dr Gee found the urea above the average, but quite within the limit of normal variation.[2] The former physician, however, in an earlier account of the disease, has remarked, "The urine during these attacks always contained a great excess of urea."[3] The difference between the urine of ordinary hæmaturia and that of intermittent or paroxysmal hæmatinuria is well expressed by Dr Harley:—"In ordinary hæmaturia, the urine is not only coagulable by heat and nitric acid, but contains blood-corpuscles, which are gradually deposited on standing; while in this form of intermittent hæmaturia, as in some cases of the non-intermittent variety, the urine, although coagulable by heat and nitric acid, contains no blood-corpuscles, and the colouring matter is not deposited on standing. Besides this, the urine contains numerous granular tube-casts, an increased percentage of urea, and a deposit of amorphous urates."[4]

As to the presence or absence of bile acids, I had suggested to Dr Affleck the importance of determining this point in the urine of hæmatinuria, keeping in view the observation of Kuhne, that the bile acids have a powerful dissolving effect on the blood-cells, and the suggestion of Dr Parkes, that some cases of hæmaturia in the urine, might be owing to an action of this kind in febrile icterus.

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[1] *Loco citato*, page 170.
[2] Ditto, page 177.
[3] Transactions of the Pathological Society of London, vol. xvi. page 175.
[4] Ditto, vol. xvi. page 168.

FIG. I.

Thyroid—carmine.

× 2 inch. 3 Eye-piece. Camera lucida. 4.

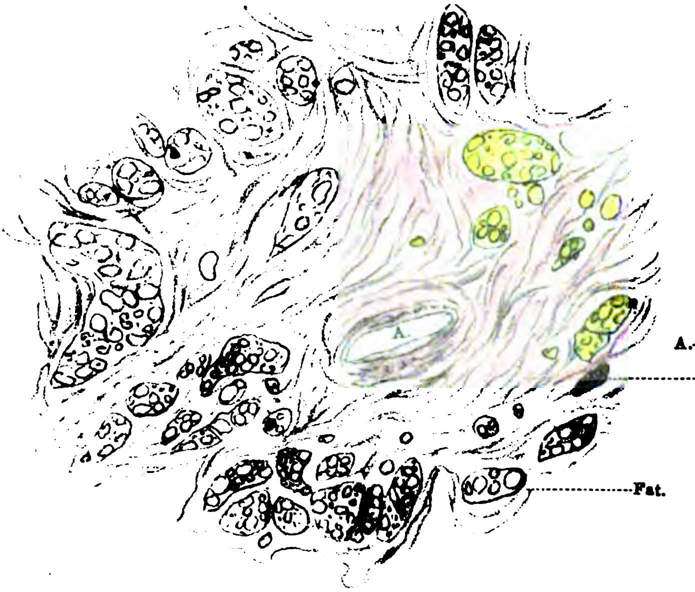
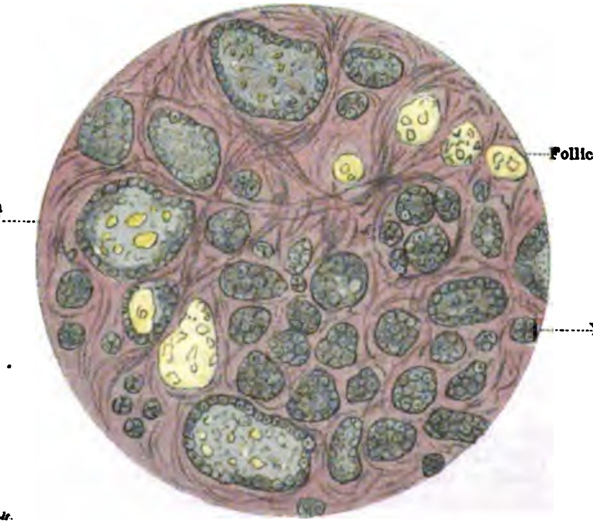


FIG. II.

Thyroid—hæmatoxylin.

× ¼ obj. 1 Eye-piece. 4.


Follicles with colloid matter.
Old follicle with colloid matter or fat.
Young follicles.

L.T. delt.

J. Bartholomew, Edin.r

On the 11th of December, the patient, whose case I have been relating, began to take quinine in five-grain doses, thrice daily. He had formerly, while under the care of Dr Menzies, been treated with gallic acid, in doses of 15 grains, thrice daily; but this astringent, so useful in many cases of ordinary hæmaturia, had in no measure influenced the discharge of blood pigment.

A few days thereafter, no appreciable change having taken place, the dose of quinine was elevated to 7 grains, thrice daily, before meals, and in addition, the patient was ordered 10 grains of the citrate of iron and ammonia an hour after meals. The effect of this treatment was apparently to lessen the amount of blood-colouring matter in the urine, and for two days together, on one or two occasions, the dark colour entirely disappeared. While continuing the medicines, the hæmatinuria returned during the very cold weather which immediately preceded last Christmas; still its occurrence was limited to the afternoons; and when the patient confined himself entirely to the house, it did not appear. Rather unpleasant symptoms of cinchonism having presented themselves, while it was evident the remedies had failed to check the discharge of hæmatin, I ordered, on the 28th of December, 20 grains of the chloride of ammonium, sal ammoniac, to be taken, simply dissolved in water, thrice daily. On the 20th of January 1875, I found that from the day on which this medicine was commenced, no dark urine had been passed. In connexion with this observation, however, it must be kept in view that the atmospheric temperature, which had, previous to the 1st of January, been very low, then rose, and milder weather continued to prevail. After this date, the patient has on two occasions omitted the remedy, with the effect of the hæmatinuria returning. I had prescribed the tincture of the perchloride of iron in 15-drop doses, thrice daily, and while taking this medicine the dark urine reappeared. It should be mentioned, that when the hæmatin was absent from the urine, the characters presented by the secretion were, as nearly as possible, those of health; and, in particular, there existed no coagulability with heat or nitric acid.

On no occasion did I observe the hæmato-crystalline, although very diligently looked for, on account of Sir William Gull's observation. "The granules," remarks Sir William, "when carefully examined, are found to consist chiefly of very small prismatic crystals of hæmatin; and even such granules as are not so distinctly crystalline are, on changing the focus, seen to have a somewhat crystalline appearance." The minute granules in the case I have described were at no time crystalline in appearance.[1]

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[1] This seems a disease in which the employment of Mr Mahomed's interesting test of the presence of blood in the urine in minute quantity, or rather of the crystalloids of the blood, might with some advantage be made. By a careful use of the guaiacum and ozonic ether test, infinitesimal traces of blood can be detected in the urine. See "The Etiology of Bright's Disease and the Prealbuminuric Stage," by Fred. A. Mahomed, M.R.C.S., Medico-Chirurgical Transactions, vol. lvii.

The case now briefly narrated is the second instance of hæmatinuria with which I have met. An earlier one came under my notice in February 1873, having been sent to me by Dr Macleod of Hawick. From Dr Macleod I have received the letter written to him after examining this patient, a gentleman of upwards of fifty years of age. I now take the liberty of transcribing certain portions of that communication:—"Mr H.'s case is one of great interest. No urines could be more different than the two specimens he brought with him to-day, voided within a few hours of each other: the one normal in its character, containing no trace of albumen or blood; the other of a deep porter colour, due to the presence of blood-colouring matter. The blood no doubt comes from the kidney. Under the microscope there are visible granular casts of the tubuli uriniferi, a few crystals of oxalate of lime, and some of uric acid; also much amorphous and granular matter, but no blood-discs. The latter circumstance is of much significancy, and allies the case with the so-called hæmatinuria—that is, the passage in the urine of hæmatin as distinguished from hæmaturia. I do not think there exists any evidence of renal calculus (of the existence of which malady the patient himself was suspicious), and I believe that the affection in Mr H. is connected with the ague, or feverish disorder resembling ague, from which he suffers. The presence of blood in the urine, Mr H. tells me, is generally to be traced to exposure to cold, while warmth wards off the attack. I think Mr H.'s case less serious on account of its being hæmatinuria. He must be very careful in avoiding exposure to cold and fatigue; he ought to clothe warmly, wearing flannel next the skin, and particularly across the loins; live generously, but on simple food, not eating much animal food, and drinking only light wine. Will you order for him thirty drops of the liquor ferri per-nitratis, to be taken thrice daily after meals, with four minims of the liquor arsenicalis? After a little time, full doses of quinine may be tried; and in the event of any severe or protracted attack occurring, I should wish Mr H. to take the oil of turpentine, in doses of twenty or thirty drops, repeatedly. I think you may speak encouragingly to Mr H."

Dr Macleod subsequently informed me that this gentleman had his first attack of hæmatinuria about eight years before his visit to me, and that it occurred after a severe chill or rigor when seal-shooting in the Hebrides. Subsequent attacks took place under circumstances somewhat similar; one in particular, in autumn weather, when exposed to cold in the coursing field. Mr H. now resides in the north of England, and has from time to time consulted Dr William Murray, of Newcastle. From the patient himself I had recently the following interesting letter:—"I cannot say I am any better. The attacks now are brought on by a much slighter exposure to cold, and consequently are more frequent; I do not suffer the same amount of pain, nor have I the shivering fits as formerly, but I am altogether weaker. I seldom go out since the cold weather set in, and only in a covered conveyance; still, even with this precaution, I am sure to suffer from an attack of more or less severity, my urine at the time varying in colour from pale coffee to blood. During last summer, for about two months, when the temperature ranged about 60°, I was free from attacks, as I am now when I keep in the house at 60°. I have said that the extreme pain and shivering have not accompanied late attacks, and sometimes I am not sure in slight ones whether it is an attack or not until I attempt to make water; then I can tell at once, by the penis being drawn back almost entirely into the body, if the water is to be ever so little discoloured. I think I did not mention this when I saw you, nor am I sure that I had then noticed it. I had an attack of jaundice last May."

The last statement is one of importance. There is some connexion, I believe, between liver disorder and the passage of hæmatin in the urine. Dr Roberts remarks—"In most cases the patient has presented a somewhat sallow and icteric aspect, or has looked pale and sickly." Dr Harley's first patient was slightly jaundiced, "as a result," Dr Harley adds, "most probably, of the malarial poison, from the effects of which he had not as yet entirely recovered. The varying condition of the three urines," the same writer observes, "clearly pointed to intense congestion of the chylopoietic viscera of a transient and periodic character. Fitting the practice to the theory, mercurials, and afterwards quinine, were taken by this gentleman, in order to remove the congestion of the chylopoietic viscera, and check the periodicity of the disease. The results were most favourable; for although four years have passed away, he has never had a recurrence of these urinary symptoms." In Dr Harley's second case, the sallowness which the patient exhibited appeared to be due to some disturbance of the hepatic functions. The man admitted that he was a very bilious subject. Similar treatment resulted in the patient's complete recovery. The notable connexion of liver disorder with hæmatinuria, just like that of ague, while seen in certain instances, has not been observed in any considerable number of the recorded cases. The influence of cold is, however, universal; and this remarkable circumstance has in some individuals been observed, that "the hæmorrhage," to use Dr Dickinson's words, "has always ceased on the removal of the cold which produced it, and has recurred with undiminished readiness on the next exposure."[1]

Dr George Johnson, it is well known, has called particular attention to the circumstance of temporary albuminuria following cold bathing.[2] Recently the subject of albuminuria, hæmaturia, and hæmatinuria, in their relation to thermic neuroses, and to taking cold, has received a most interesting illustration by Dr Laycock.[1]

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[1] *Op. cit.*, page 179.

[2] British Medical Journal, vol. ii. for 1873.

In the paper to which I refer, Dr Laycock has given some instructive examples of transient neurotic albuminuria, and he has pointed out that there is much in common, in certain cases, between albuminuria and hæmaturia, and, it may be added, hæmatinuria. Dr Laycock believes, and he appeals to facts in support of this belief, that "in the process known as taking cold, there is always a change induced in the trophic nervous system [this does not, in his view, mean the vaso-motor system only, but that larger system which, including the vaso-motor as a higher and special evolution of it, presides over the primary or vegetative organic processes of nutrition, so as to modify the chemical conditions of the blood-corpuscles, lymph, and tissues in general, as well as the contraction of the bloodvessels and the distribution of the blood], both locally and generally, such that one or other of a numerous group of trophic changes result in organs and tissues, and that this morbid and morbific change is one of the primary and most essential conditions of the process."

There can be little doubt that, in the curious disease which we are considering—a person apparently in his usual health, on being exposed to cold, even to a modified degree of cold, passing bloody urine, which phenomenon ceases for the time when the individual is placed in a higher temperature—an injurious impression is made on the nervous system by the cold, and it is when the surface of the body is chilled, that the kidney disturbance is effected. That the renal function should be so seriously, although only temporarily, influenced, as to determine the passage of either albumen, blood, or blood colouring matter, it may, I think, be presumed, and more particularly in the latter case, which we are considering, that some morbid change in the blood itself, although it has as yet eluded detection, has existed; some product, probably, of faulty hepatic function being the "fons et origo mali." According to our present knowledge, unquestionably defective, I think it very probable, that in paroxysmal hæmatinuria the primary morbid change takes place in the blood. The kidney, however, speedily suffers. Sir William Gull, in the course of some interesting and suggestive observations on the pathology of this disease, specifies "pains in the loins," as showing, along with the remarkable urinary changes, that the kidneys are affected. But lumbar pain is by no means a necessary, I doubt if it be more than an occasional, concomitant of the disordered secretion; while the appearance the urine presents, and its features, when carefully examined, are quite sufficient to establish the implication of the kidney. The kidney forms the hæmatin, the colouring matter of the urine; and in order to accomplish this important function, the organ must be in a tolerably healthy condition. Sir William Gull illustrates this by a reference to what occurs in certain cases of post-scarlatina nephritis. "We frequently," he says, "see hæmatinuria as a sequela of scarlet fever. The usual history is as follows:—After an attack of scarlet fever the child passes albumen and blood in its urine. The microscope shows that blood-corpuscles are present. The affection is a simple hæmorrhage from the kidneys. But when the child advances towards recovery, and the kidneys begin to resume their functions, although albumen may be present in the urine, we no longer find blood-corpuscles, these being replaced by hæmatin. The urine is dusky, but contains no blood-corpuscles. The kidneys have regained their functions so far that they can now break up the blood-corpuscles. In the next stage the urine, still containing albumen in small quantities, presents uric acid and urates, and we then know that the kidneys are beginning to recover themselves. In the fourth stage the urine contains no albumen, but urates, urea, and its natural colouring matter. The kidneys have then totally regained their functions, and we have seen, step by step, the dynamical power of these organs return."

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[1] On Neurotic Albuminuria and Hæmaturia. Dublin Journal of Medical ce, July 1874.

The analogy between the urine thus affected and hæmatinuria as it occurs in cases such as those I have described, is a tenable one. It is not of course perfect, for in the cases we have been considering, the uric acid excreting function of the kidney is preserved, and, as we have seen, the power of discharging urea may even be increased. Still, the function of converting hæmatin into normal urinary pigment is interrupted; or, as Sir William Gull expresses it, the kidneys "ought to eliminate the hæmatin in the condition of urine pigment; instead of that they eliminate the hæmatin itself."

As to treatment, warmth is evidently the remedial agent of the highest efficiency in this disease. It is alike preventive and curative. In many cases the attack is warded off so long as the surface of the body is kept warm; and in all instances on record, the restoration of heat, when that has been removed, has led to the cessation of the attack.

The astringent remedies which are so useful in the treatment of ordinary hæmaturia signally fail to exert any therapeutic action in paroxysmal hæmatinuria. This, at all events, may be affirmed of acetate of lead, gallic acid, and the preparations of iron, although the latter are indispensable in combating the resulting anæmia.

Quinine, in full doses, is unquestionably a valuable remedy. Next to heat, it may for the present be considered as the remedy of most value in paroxysmal hæmatinuria. It has, however, failed in other hands; and in the case, the details of which I have now given, it also failed, while another remedy apparently succeeded. That remedy was the chloride of ammonium—the sal ammoniac. It would be very absurd to extol the virtues of the muriate of ammonia in paroxysmal hæmatinuria on the strength of one case only; but I am anxious to bring its employment to the notice of the profession, being very hopeful of its being found serviceable in this as well as in other diseases of neurotic origin.

I was led to employ it in the case now narrated from reflecting on the very remarkable deobstruent and alterative properties which that salt possesses, more especially in relation to hepatic disorders, and, further, from its frequently great power in relieving neuralgic suffering.

Acting directly on the nervous system and on the blood, it appeared to me a medicine, likely to exert a beneficial influence in a disease, in which disturbance of nervous system and of blood unquestionably coexisted. In this expectation I have not been disappointed in the only case in which I have as yet had the opportunity of employing it.

ARTICLE VII.—*Clinical Lectures on Diseases of the Heart.* By GEORGE W. BALFOUR, M.D., F.R.C.P.Ed., Physician to the Royal Infirmary.

On Curable Mitral Regurgitation; its Various Causes, the Physical Signs by which it is revealed, and its Treatment.

GENTLEMEN,—In a former lecture[1] I pointed out that the only unequivocal proof of the existence of actual disease of the mitral valves, is the determination of the existence of an auriculo-systolic, or, as it is commonly termed, a presystolic murmur, or, at all events, sufficient proof of the presence of such a murmur at some former period of the patient's life; for though, as is well known, this murmur may frequently disappear or alter in character, the lesion upon which it depends is permanent. My statement would have been more correct had I said that the only unequivocal proof of disease of the mitral valves is the determination of a constricted condition of the auriculo-ventricular opening, however that may be established. For constriction of that opening is, except in the rarest instances, unfailingly present when the mitral valves are diseased, and though it is always present when a presystolic murmur exists,[2] it also frequently occurs when that murmur is not only absent at the time of examination, but has never been recognised at any past period of the history of the case, though even in these instances its existence is revealed by certain signs to which I have already referred.[1] As in that lecture, however, I referred chiefly to the murmur distinctive of disease of the mitral valve, the statement may be accepted as correct to that extent. At present I intend to refer to mitral regurgitation unaccompanied by disease of the valve, and shall have to traverse cursorily a wide series of diseases in which this lesion is found present more or less frequently, and in a large proportion of which it is happily permanently curable, even though revealed by what to many even yet seems the certain sign of an incurable organic cardiac disease—a mitral systolic murmur. The importance of this observation needs no comment. To the patient it involves the issues of life and death, a comparatively short illness, and thereafter a normal and it may be a long life; or permanent ill health, an abnormal life, and shortened days. To the physician it brings home the paramount importance of an accurate diagnosis, and substantiates in a most striking manner the value of appropriate treatment.

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[1] Ed. Med. Jour., Nov. 1871, p. 431.

[2] I have no hesitation in making this statement, notwithstanding that Professor Flint says, "I have recorded three cases in which this (presystolic) murmur was loud, and the mitral valves were found post mortem normal. In each of these cases there were aortic lesions rendering the aortic valves insufficient."—Lectures on Diseases of the Heart, Philadelphia, 1870, p. 207. Obviously Flint had not timed these murmurs correctly, for a murmur depending on aortic regurgitation is always diastolic, and therefore has a rhythm very different from a presystolic murmur. But even without timing these murmurs, they have each of them such a distinctly different character that no one per- tly acquainted with the presystolic murmur could ever mistake a diastolic c murmur for it.

Daily experience in the sick room has long since taught all of us that muscular debility and relaxation are the inevitable results of all febrile diseases; theoretically we know that the heart is a muscular organ, that even in sickness it never rests, and that the influence of malnutrition—unrepaired waste—so evident in the other muscles, must be even still more predominant in it. But how few of us have recognised the different modes in which this malnutrition and relaxation of tissue may make itself known. When diminution of the mass of blood accompanies an absolute wasting of the tissues, as in phthisis, the heart simply atrophies in all its dimensions, and its cavities are correspondingly diminished in size (concentric atrophy), and so invariable is this the case that one eminent author (Walshe) states that the sustainment of the heart's weight at the par of health in those slowly cut off by such exhausting diseases as phthisis or carcinoma must be regarded as the equivalent of hypertrophy in those free from such diseases.[2] In the case of other diseases, some of a more distinctly febrile character, such as typhus, enteric, and relapsing fevers, erysipelas, and probably many others, as well as in others of even a less febrile type, such as chorea, and some forms of anæmia, especially chlorosis, the change in the condition of the heart consists not in any tendency to atrophy, but rather to such a complete relaxation of its tissues, that in its most extreme form it seems as limp as a piece of wet paper, yet without any very evident diminution in its bulk, or any marked alteration in the relative dimension of its cavities. This altered condition of the muscularity of the heart is always preceded and accompanied by a more or less evident alteration in the blood. It may be diminished in quantity—anæmia; it is always more or less altered in quality—spanæmia. And it is, I believe, to the predominance of one or other of these hæmic lesions, as well as to the comparative amount of cardiac relaxation—and consequent debility—present, that we are to attribute the constant prevalence in certain diseases, such as chlorosis, of certain peculiar phenomena which are only occasionally found in other diseases in which muscular relaxation and blood alterations are also present, whether these diseases are febrile or non-febrile in character.

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[1] Ed. Med. Jour., Jan. 1872, p. 615.

[2] On Diseases of the Heart, 3d ed., p. 274.

As the cardiac and hæmic phenomena to which I refer are most typically developed in chlorosis,[1] I shall first of all direct your attention to the character and the explanation of these phenomena as they are found to occur in that disease, and thereafter employ the information thus obtained in the explanation of similar facts occurring in the course of other diseases. And in doing so, I shall not particularly enter into the pathology of chlorosis generally, but simply describe it as a form of spanæmia of common occurrence, especially in country girls who for the first time find themselves exposed to the unhealthy surroundings inseparable from domestic service in a town, and in whom the prominent derangement of the genital functions is almost invariably amenorrhœa. It is also of no infrequent occurrence in town-bred girls of a better class, especially about the time of puberty, and in them the menstrual discharge is pale, usually scanty—menorrhagia being exceptional—and always followed by great exhaustion. In such patients the lips externally may be rosy, and the cheeks present a certain amount of youthful bloom, but the interior of the lips and the gums, but especially the conjunctivæ of the lower eyelids, are pale and bloodless. The face is rather puffy than clear cut or sharp in its outlines; the ankles tend to swell, the appetite is irregular and defective, and there is breathlessness and palpitation on the slightest exertion. This condition is also signalized by the occurrence of what are termed functional or hæmic murmurs, which are of constant occurrence in the veins, forming the well-known *bruit de diable*, or humming-top murmur, and are not unfrequently heard in the aortic area, more frequently in the pulmonary area, and more rarely in the mitral area. The venous murmur is continuous and audible, both during systole and diastole, in equal intensity. The cardiac murmurs are always systolic in rhythm.

Much of the interest of these murmurs centres, as you may well believe, in their causation. If we regard them as purely hæmic in character, then, however difficult it may be to understand their production, their removal is as simple as we know it to be easily produced, and possessed of no great general pathological importance. But if we can show, as I hope to do, that the cardiac portion of these murmurs is not simply hæmic or purely functional in character, but is the product of an actual cardiac lesion, then we link blood disease with cardiac lesion in a very striking manner, and obtain the important pathological information that some cardiac lesions are certainly and permanently curable.

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[1] I refer here to the ordinary and curable form of chlorosis commonly met with, and not to that incurable form, accompanied by hypoplasia of the internal organs, referred to by Virchow in his pamphlet, "Ueber die Chlorose und die damit zusammen hängenden Anomalien im Gefässapparate," etc. Berlin: Hirschwald: 1872. And also by Rokitansky, in his "Handbuch der Pathologischen Anatomie," Bd. i. s. 158, and Bd. ii. ss. 418 and 585. 1840-44.

I need not test your patience nor occupy your time with an account of all the various theories which have been propounded in explanation of the production of venous murmurs. It is enough that the modern science of physics has proved that sound in relation to fluids is always caused by the production of Savart's fluid veins at the point where it is heard.[1] Now, venous murmurs are readily produced in spanæmic individuals by moderate pressure over any vein of tolerable size, such as the external jugular. They are also heard over the torcular Herophili, over the upper part of the innominate veins, especially the right one, and over other parts of the venous circulation where there exists naturally what is produced by pressure, an escape of blood through a part which is relatively constricted to that beyond it. Besides relative constriction, however, a certain amount of force is requisite to produce these sonorous veins. Hence they are frequently inaudible in the recumbent position, becoming at once audible when the venous flow is hastened by elevating that part of the body at which we may be listening. It is true, as has been noticed by many observers, that venous murmurs are frequently audible in individuals who are in apparent health; but we are always oscillating between health and disease, and are regarded as healthy so long as we keep within certain well-defined limits. All that this observation proves, therefore, is, that a certain amount of spanæmia is not inconsistent with apparent health; it by no means proves that venous murmurs are compatible with perfect health, or, what is synonymous, with perfect blood. As yet we cannot accurately define what is the real connexion between spanæmia and venous murmur; but knowing as we do that it is produced in certain positions, as at the torcular Herophili, where no other change is at all probable, under all the circumstances of the case, except one involving an altered relation of the blood to its containing vein, we can have no hesitation in ascribing it to that cause. And, further, from what we know of the mode of production of fluid veins, we can have no difficulty in saying that this altered relation of the blood to the walls of the vein must consist in the production of increased friction between the two, so that in those positions where there is normally a relative constriction, insufficient, however, to produce sonorous veins, the increase of friction between the wall of the vein and the layer of blood next it practically narrows the opening sufficiently to do so. Physical laws leave no doubt as to this. It is for physiologists

[1] Ed. Med. Jour., June 1874, p. 1086.

to explain whether it depends simply upon a watery condition of the blood or upon some other cause, and their explanation would no doubt be of the greatest importance for practical medicine.

But the venous murmurs which accompany chlorosis are for many reasons of very much less interest and importance than those heard in the cardiac area. As already mentioned, these are systolic in character, and are most frequently heard in the pulmonary area, less frequently so in the aortic area, and most rarely in the mitral area. These arterial bruits have been supposed to be due to a diminution of the arterial tension (Marey), facilitating the production of sonorous veins at the arterial orifices. But we need but little experience in clinical medicine to convince us that diminution of arterial tension is not sufficient to account for the production of a murmur; and it is still more unfortunate for this theory that this bruit is always best marked, and often solely to be heard, over the pulmonary artery, where the well-marked accentuation of the second sound assures us, that so far from there being any diminution, there is actually an increase of the arterial tension. But it is a still more curious fact, that by carefully examining all the cardiac areas, we discover that the position of maximum intensity of this so-called arterial bruit is not over any artery at all, nor in any of the usually accepted areas of cardiac sounds and murmurs, but is actually about one inch and a half, or rather more, to the left of the pulmonary area, and in the same plane, immediately over the part where the appendix of the left auricle pops up from behind, just to the left of the pulmonary artery. This so-called arterial murmur is, therefore, not arterial at all, but strictly auricular in its source. This is a most important fact; it at once links this chlorotic murmur with others whose source we distinctly know, and gives us an unmistakable clue to its true point of origin. We all know very well that when the first sound in the mitral area is replaced by a murmur, that this murmur is propagated with diminished intensity into the pulmonary and also into the aortic areas. But it has also been observed that this systolic pulmonary murmur accompanying mitral disease is occasionally actually louder than that audible in the mitral area, and is sometimes only to be heard in the first-mentioned area. Skoda[1] was the first to point this out, and he attributed it to a hypothetical alteration of the lining membrane of the pulmonary artery in such cases. Joseph Meyer[2] has shown that this hypothetical alteration is a myth, and has explained the occurrence of this phenomenon by another equally untenable supposition. But it was reserved for Naunyn[3] to show that this murmur of mitral disease, whether it accompanies a systolic murmur in the mitral area, or is itself the sole indication of such disease, has its position of maximum intensity, not in the pulmonary area, but one inch and a half or more to the left of the left edge of the sternum, in the second interspace; that is, as he has shown, and as anatomy teaches us, not over the pulmonary artery at all, but exactly over the spot where the appendix of the left auricle comes up from behind, just to the left of that artery. Naunyn explains this phenomenon by attributing it to the better conduction of the murmur along the course of the regurgitating blood, the fluid veins producing sonorous vibrations louder at the point of impingement than at that of origin; and to the circumstance that the dilated auricle in all such cases is closer than usual to the anterior surface of the chest, and therefore nearer to the ear.[1] This condition of things is most common in mitral constriction, and there are other circumstances which in this state contribute materially to the production of this phenomenon. At present, what I wish to point out is, that in chlorosis, in which all these circumstances, to which I may now comprehensively refer under the head of cardiac dilatation as consecutive to spanæmia, exist only in the very slightest degree, this pulmonary or rather auricular murmur is often—so often as to constitute it almost invariably—the sole sign of mitral regurgitation in such cases, and that therefore, while we accept its position of maximum intensity as a clear indication of the mitral origin of this murmur, we must also acknowledge the propagation upwards into the auricle of this mitral murmur, in the absence of any great or well-marked auricular dilatation, as an unmistakable proof that there is something in the physical constitution of the murmur—possibly in its pitch, more probably perhaps in the direction of the fluid veins—which makes it physically more capable of propagation in the direction of the appendix auriculi than downwards to the cardiac apex, and in this the relative densities of the auricular and ventricular walls and their relative tension must play no unimportant part. The position of maximum intensity of this chlorotic murmur over the appendix of the left auricle connects it very distinctly and unmistakably with the phenomena observed in certain cases of mitral constriction on the one hand, and with the murmurs of dilatation of the left ventricle on the other, all these being alike dependent upon mitral regurgitation. While, if these proofs of the dependence of the so-called pulmonary murmur of chlorosis upon the same cause be still considered insufficient, further and more conclusive evidence is to be found in the fact that all the ordinary murmurs are not unfrequently found associated in chlorosis with a true murmur of mitral regurgitation distinctly and clearly audible in the mitral area, and accompanied by distinct accentuation of the pulmonary second. While, as if to show the interdependence of these murmurs upon one central lesion, we find them disappear in the exact order in which we would expect them to do so—first the mitral murmur vanishes, then the murmur in the aortic area ceases, next the auricular murmur disappears, and last of all the venous murmur becomes inaudible. In the person of Helen Christison, Ward XIII., bed 9, admitted 12th February 1875, you have had an excellent opportunity of observing what I have just described, the gradual and consecutive disappearance of all the murmurs mentioned, coincident with the restoration of the patient to perfect health. The central lesion, so far as the circulation is concerned, upon which all these phenomena depend, is certainly an alteration of the blood revealed by a venous murmur. You are all of course aware of the statements of the "London Heart Committee," of Liman, and of Winterich, to the effect that venous murmurs are of frequent occurrence in individuals apparently healthy. There is no reason to dispute this statement, identical physical causes invariably produce the same results, the whole science of physics is based upon this axiom; when, therefore, these observers acknowledge that in a certain percentage of observed cases this venous murmur is wanting, they also acknowledge that they have been observing two distinct sets of cases, one or other of which—but not both—has healthy blood. For myself, I can only say, that constantly examining as I do the jugular veins in all kinds of cases, I have frequently found this murmur in persons having the appearance of health, but I have also found it always removable by appropriate treatment, and have thus been led to regard it as always a sign of a morbid condition of the blood—a condition of the blood which is often only morbid in that it is not healthy, but one which if prolonged or excessive leads to serious deterioration of health, which exposes the patient to a thousand different ailments, and which frequently and often apparently suddenly culminates in what is not unfrequently mistaken for serious cardiac mischief.

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[1] Abhandlung über Percussion und Auscultation. Wien, 1844. s. 233.

[2] Virchow's Archiv, Bd. ii. heft 2, s. 277.

[3] Berliner Klinische Wochenschrift, 1868, No. 17, s. 189.

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[1] This peculiar phenomenon has also been referred to by Bamberger, Friedreich, and others, and explained after different methods. Recent writers have all, however, adopted Naunyn's explanation —*Vide* Paul Niemeyer, "Handbuch der Percussion und Auscultation," Bd. ii., abtheil i. s. 140, Erlangen, 1870; and Gerhardt's "Lehrbuch der Auscultation und Percussion," 2d edition, Tübingen, 1871, s. 283.

Slight venous murmur is often present without other signs of mischief, but when at all developed it is invariably associated with an auricular murmur, and therefore with some degree of cardiac dilatation. Chlorotic murmurs in the mitral area are more rare, frequently they amount to little more than an impurity of the first sound, but occasionally they are exceptionally loud and distinct, and if associated with a previous history of rheumatism, it may be impossible to decide—apart from the results of treatment—whether the murmur is due to chlorosis or to actual valvular disease. The following case affords a striking instance of this:—

CASE 1.—Elizabeth Heughson, a domestic servant, aged 19, admitted to Ward XIII. on 28th March 1873, examined 29th March. The patient is a middle-sized girl, of fair complexion and anæmic appearance. About two and a half years ago she suffered from rheumatism and pain over the cardiac area; since then she has been liable to occasional rheumatic pains in various parts. She has been much exposed to cold and damp, from the condition of her abode in Oxford, and from having had to take part in the family washing. Her present illness came on in November last as simple weakness, which since then has been gradually increasing. She has not menstruated for the last two periods; menstruation previously had been scanty. Except the circulatory, the other systems are normal, only her digestion feeble, and her abdomen flatulent. The cardiac apex is felt to beat somewhat diffusely between the fifth and sixth ribs, two inches and three-quarters from the left edge of the sternum. The cardiac dulness, one inch from the left edge of the sternum, commences at the upper edge of the third rib and extends downwards to the liver dulness; at the fourth rib the transverse dulness begins one quarter of an inch to the right of the sternum, and extends across for a distance of five inches and one quarter. In the mitral area there is a loud blowing systolic murmur, which is propagated entirely round to beneath the angle of the scapula, and is followed by an accentuated second sound. In the aortic area the first sound is replaced by a murmur. In the pulmonary area the first sound is also replaced by a murmur, which is loudest at one inch and a half from the left edge of the sternum, and is followed by a distinctly accentuated second sound. The pulse is 108, small and feeble; a loud bruit de diable is audible in the jugular veins of the right side. She was treated by various preparations of iron, at first with digitalis, latterly in the form of the ammonio-citrate without digitalis. Under this treatment she improved in strength and regained a more healthy appearance, her palpitations almost entirely ceasing, but her murmur remaining the same. On the 23d of May she was sent to the Convalescent Hospital, whence she returned for examination on the 19th of June, when she was found to have completely regained all the appearance of health; her cardiac murmurs had entirely disappeared, the accentuation of the pulmonary second sound was gone, and the venous hum had also ceased. This case was one in which, though the chlorosis was well marked, yet the history of rheumatism was so distinct that we dared not hope for complete restoration. The result of the treatment showed that the valves had been quite unaffected, and that the whole of the very striking phenomena had been due to simple dilatation of the left ventricle, the result of chlorosis; the absence of any remaining accentuation of the pulmonary second sound conclusively proving that this was a case of actual cure by contraction of the cardiac cavity, and not simply a case of the vanishing of a murmur, the lesion remaining.

I need not say how frequently during the course of acute rheumatism we find a systolic mitral, an auricular so-called pulmonary, and an aortic murmur developed—the mitral sometimes alone, the auricular occasionally by itself, but never an aortic murmur without one or other of the before-mentioned murmurs coexisting, all these phenomena disappearing on the return of health. After what has been already said, the reason for this is obvious; the defective nutrition of the cardiac muscle, depending on the depraved constitution of the blood, has resulted in relaxation of the muscular tissue and dilatation of the ventricular cavity, this producing imperfect closure of the mitral valve, and hence a mitral murmur which has its position of maximum intensity at one time in the mitral area, and at another in the auricular area, according to circumstances which have been referred to, but which cannot as yet be adequately explained, while these murmurs are propagated in greater or less intensity into the pulmonary and aortic areas. When the normal condition of the blood is restored, the cardiac muscle recovers its tone, the mitral valve again closes perfectly, and all signs of a cardiac affection disappear. Should the fibrous tissue of the valves or of the cordæ tendineæ become thickened or shrivelled as the result of the rheumatic process, then all the signs of incurable mitral disease become developed; sometimes uninterruptedly consecutive to those of the curable form; at others, the curable form of the disease is temporarily recovered from, and the incurable form subsequently developed after an uncertain interval of apparent health, sometimes apparently as the result of the primary rheumatic affection, at others, more evidently caused by the persistence of the rheumatic disease in a chronic, sub-acute, or chronically recurrent form.

Mitral murmurs of a curable character are frequently found associated with chorea, and though, from the close connexion which subsists between this disease and rheumatism, murmurs originating during its course occasionally become permanent and incurable, yet in a large proportion of these cases the murmurs are curable, and permanently disappear after the patient's restoration to health. In such cases it has been usual to ascribe the occurrence of these mitral murmurs to irregular action of the heart, the result of clonic spasm of some part of the cardiac muscle, particularly one or other of the musculi papillares, similar in nature to the clonic spasms which in this disease affect the voluntary muscles. Against this theory, however, we have the facts, that while the theory of clonic spasm of any part of the cardiac muscle might readily account for an intermittent murmur, it is not sufficient to account for a mitral murmur, which, while it lasts, is constant and unchanging; for such has been its character in my own experience, and I know of no recorded observations which controvert this view. On the other hand, the same depravation of the blood, and consequent relaxation of the muscular tissue, which is so marked in chlorosis and acute rheumatism, is even more exquisitely developed in chorea; hence, there is no difficulty in referring the continuous mitral murmur in chorea to the same cause which has been already shown to be capable of producing it in those diseases referred to.

There is hardly any form of febrile disease in which so-called hæmic cardiac murmurs have not occasionally been heard. In scarlatina, relapsing and enteric fevers, they are of common occurrence; in erysipelas, smallpox, measles, and probably in all other febrile diseases, they are occasionally heard; while in true typhus such murmurs are somewhat rare.[1] When we consider that relaxation and softening of all the muscular tissues, both voluntary and involuntary, is common in all protracted forms of febrile disease, and when we also reflect that this alteration of the muscular tissue depends mainly upon altered nutrition, and must therefore vary considerably according to the previous condition of the patient in this respect, we see sufficient reason why in ill-nourished and anæmic patients a less protracted disease should give rise to phenomena which in better nourished patients are only occasionally observed, even when they have been exposed to a more severe and protracted disease. Hence the want of constancy in the cardiac phenomena referred to in every disease; hence, too, their probably greater frequency in hospital patients. The alleged infrequency of such murmur in typhus patients is readily understood, when we remember that to produce sonorous fluid veins a certain amount of propelling force is requisite, and remember, also, that the cardiac muscle is frequently so enfeebled in this disease that murmurs due to actual disease of the valves disappear during its course, and reappear as the patient convalesces and the heart re-acquires sufficient force.[2] Probably enough, careful auscultation of the auricular area in every case of typhus would reveal a systolic murmur there as the first and earliest indication of failing cardiac power; if this be not the case, the absence of this murmur can only be referred to the prevalence of anæmia over mere spanæmia in most cases of this disease—a condition exactly the reverse of that found in chlorosis.

In all febrile diseases, whatever their character, the sequence of the phenomena, as observed by myself, has been precisely similar to that observed in chlorosis. That is, first, we have had the venous hum; second, the auricular murmur, propagated into the pulmonary and aortic areas, either as an impure and prolonged first sound, or as a distinct systolic murmur; and, lastly and more rarely, a systolic murmur in the mitral area. And I may shortly recapitulate my reasons for believing that these murmurs, as existing in the cardiac area, are produced by actual regurgitation through the mitral valve, and are neither arterial in character nor produced simply by the unnatural friction of the blood passing along its normal course. You are aware that for the production of sound by the movement of watery fluids, the formation of what are termed fluid veins is required. These fluid veins may be formed at any part of the circulatory system where any real or relative constriction exists or may be produced, and to make these fluid veins sonorous, all that appears to be requisite is, first, that the difference between the part of the vessel constricted and that which lies beyond it should be sufficiently well marked; and, second, that the blood should penetrate this real or relative dilatation with adequate force. No real constriction has, however, been found at the arterial openings in any of the cases referred to; and in non-fatal cases, whenever the blood is restored to its normal, all bruit disappears, which would not be the case had either the constriction or dilatation any real existence. At those parts of the veins where a natural dilatation exists, the increased friction of slowly flowing spanæmic blood favours the formation of more rapid and therefore sonorous veins in the centre of the current; but such a mode of formation is not to be thought of at the arterial orifices, where the blood is thrown forwards with so much force by the ventricles as completely to annul the trifling increase of friction and prevent the formation of central fluid veins. Marey[1] has supposed that the normal difference between the aortic orifice and the aortic lumen is sufficient to account for the production of sonorous veins, when the rapid ingress of the fluid is favoured by low arterial tension. But it is needless to criticise this view, because it is insufficient to explain the facts; for if diminished arterial tension be required to account for the formation of sonorous veins at the aortic orifice, how are we to explain their formation at the orifice of the pulmonary artery, where the tension—as proved by the marked accentuation of the pulmonary second sound—is always extreme; and yet, if we believe these bruits to be of arterial origin, these sonorous veins must be more readily and more effectively formed when the arterial tension is high than when it is low, because so-called hæmic bruits are always louder and better marked in the pulmonary than in the aortic area, and are sometimes only to be heard with distinctness in the former position. This *reductio ad absurdum* completely disproves Marey's theory, and renders any arterial origin of such murmurs in the highest degree improbable, while this improbability is greatly increased by finding as we do that the actual position of maximum intensity of this murmur is not in any arterial area at all, but quite one inch and a half to the left of the pulmonary area, just over the appendix of the left auricle. But a bruit over the appendix of the left auricle, as Naunyn has shown, is the result of mitral regurgitation; that it is so in this case there is every reason to believe, first, because there is no other probable or efficient cause for the production of such a murmur; and, second, because this systolic auricular murmur is not infrequently associated with a systolic

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[1] For a history of the state of the heart in typhus and other fevers, and for an account of the murmurs which occur during their course, I beg to refer to Dr Stokes's admirable chapter "On the Condition of the Heart in Typhus Fever," in his work on "Diseases of the Heart and Aorta," Dublin, 1874, p. 366.—And for an account of the morbid changes which are found in both the voluntary and involuntary muscles of fever patients, I may refer to Zenker's Memoir, published at Leipsig in 1864, or to Murchison, on "The Continued Fevers of Great Britain," London, 1871, pp. 248-9.

[2] Stokes, *op. cit.*, p. 446; recapitulation, 17, etc.

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[1] Physiologie Médicale de la Circulation du Sang. Paris, 1863. P. 479.

murmur in the mitral area, propagated in the usual way, and which there is no reason to suppose depends or could depend upon any other cause than regurgitation through the mitral valve. The arterial murmurs are therefore not of independent origin, but are propagated from the mitral valve, which permits slight regurgitation between its segments, which are unable to close perfectly because the ventricular lumen is somewhat dilated. The reality of this regurgitation is further proved by the accentuation of the pulmonary second sound, which is always present in these cases.

You will readily perceive how important a matter this is; for if these chlorotic and febrile murmurs be actually due to dilatation of the heart, and if it be also true, as it undoubtedly is, that all, or, at all events, all but an infinitesimal proportion of such cases make a perfect recovery,[1] then we must very considerably enlarge the area of curable cardiac affections, and there can be no doubt that this is the case. I do not now of course refer to the case of simple vanishing murmurs, due to organic valvular lesions, the lesion causing them still remaining, as can be readily proved, in which, of course, there is and can be no cure, but to those numerous cases of positive dilatation of the ventricular cavities, which may range, as we have seen, from the most unimportant case of chlorosis up to more serious cases of cardiac dilatation, the curability of these being simply a question of degree. In chlorotic and febrile dilatation, cure, as we know, is the rule; in dilatation from other causes, it is perhaps the exception, but it may always be aimed at, and is not infrequently attained. In simple dilatation of the right ventricle, the result of bronchitis, the cure of the bronchitis is usually followed by cure of the cardiac affection, but this we shall recur to in speaking of diseases of the right side of the heart. In dilatation of the left ventricle from ordinary causes, usually occurring at or beyond the middle period of life, and depending upon the gouty diathesis or strenuous exertion accompanied by the free use of alcohol, the patient seldom comes under treatment till the right ventricle is also affected secondarily. Yet, even in these cases, very great improvement is of common occurrence, and in a few a positive cure—or at all events one of some permanence—is actually attained. I may give the two following cases as very interesting and instructive examples of this; the cases are of course very much condensed, only the salient points in the cardiac lesion being referred to:—

CASE 2.—Henry Welsh, a labourer, aged 61, admitted to Ward V., 17th November 1874, complaining of pain in the chest, shortness of breath, swelling of the stomach, and occasional palpitation. About thirty years ago, this patient had an attack of rheumatic fever, which he says lasted twenty weeks. For the last ten years he has had occasional pains in the left haunch and arm, which he calls rheumatic; with these exceptions, his health was perfect up to last August, when he was suddenly seized with severe pain in the front of the chest, accompanied by shortness of breath and expectoration; this lasted about a week. After this recovery, on attempting to work, he found that the slightest exertion produced breathlessness and much feeling of weakness; the pain in the chest felt at first abated, but left behind it a soreness which still continues. About a month after his illness, he first noticed swelling of his feet at night, and since then he has remained much in the same condition. The patient is a well-built powerful-looking man, nearly six feet in height; his muscles are, however, soft, and he says he has lost much flesh. Now he can lie in any posture, but till six weeks ago he required to have his head raised on account of breathlessness. Expression of face natural, skin natural, temperature 98·6°. The soreness complained of is referred by patient to a point about the middle of the sternum; it is always present, and though not very severe, it becomes worse upon any exertion. He has no dyspnœa when at rest, but on making any exertion a choking sensation is perceived, referred by him to the part where pain is felt. At first, was much troubled with palpitation, but this is not now so frequent. Pulse 92, regular, composed of alternate strong and weak beats; at times the force of each beat is tolerably uniform, but accompanied by intermissions and occasional double beats. The veins generally are not distended, the external jugulars are slightly so, and pulsate distinctly, but do not fill from below on pressure being made on them with the finger. The cardiac apex beats between the fifth and sixth ribs, four and a half inches from the mesian line; no other pulsation is visible. On palpation, the cardiac impulse is felt to be feeble and irregular, both as to time and force. On the left side, one inch from the sternum, the cardiac dulness does not rise above the third rib. Transversely, in the plane of the fourth costal cartilages, the dulness extends from half an inch to the right of the sternum to four inches to the left of the left edge of that bone. On auscultation in the mitral area a high-pitched systolic murmur is heard, accompanied in three beats out of four by a partially closed first sound; in the fourth the murmur alone is audible. The maximum intensity of this murmur is in the mitral area, but it is propagated distinctly round to the angle of the left scapula. In the mitral area this murmur is followed by a somewhat obscure second sound. In the tricuspid area a loud but low-pitched systolic murmur is distinctly audible, accompanied by some portion of a first sound, and followed by a second sound, which is sharper than that in the mitral area. At the base the first sound is closed, the pulmonary sound markedly accentuated. There is no venous murmur. This patient gradually improved under treatment, till, on his discharge on 18th February 1875, the only evidence of cardiac disease present was a slight impurity of the first sound in the mitral area, and an almost imperceptible accentuation of the pulmonary second. The tricuspid murmur had entirely disappeared.

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[1] We all know that chlorotic murmurs ordinarily result in perfect cure, and in regard to those occurring in the course of fevers, Stokes says, "Of many hundreds of cases of weak and softened hearts observed during the last twelve years, we cannot adduce a single instance of organic disease of the heart which could be traced to any injury done pending the typhous affection."—*Op. cit.*, page 374.

Time alone can prove whether in this case there was slight damage to the mitral valve, dating from his rheumatic attack thirty years ago, or whether, as seems probable, his sole lesion was dilatation of both ventricles, dating from his febrile—probably bronchitic—attack of August last. In any case, his improvement was both marked and remarkable, and the contraction of both ventricles, but particularly of the right one, unmistakable.

In the following case, the nature of the lesion is less open to question, and the improvement even more extraordinary:—

CASE 3.—Euphemia Lawson, a domestic servant, admitted into Ward XIII. on 26th September 1872, labouring under breathlessness and swelling of the lower extremities. In this case there was forcible pulsation at the lower part of the sternum, pulsation of the jugular veins, and slight increase of the transverse dulness of the heart. In the mitral area a blowing murmur, and in the tricuspid area a similar murmur, of a somewhat lower pitch, completely replaced the first sound. These murmurs were propagated into the aortic and pulmonary areas; the aortic second sound was weakened; the pulmonary second much accentuated. She never had any rheumatism whatever, but had hard work, and was rather given to the abuse of stimulants. About three months after admission, at the hour of visit, we were suddenly called to her bedside to find her completely unconscious, and paralyzed on the left side. The unconsciousness passed off within half an hour. The paralysis of the arm ceased to be noticeable in a few days; and, after the lapse of some weeks, the paralysis of the leg was only revealed by a slight dragging, which ultimately quite disappeared. Under treatment, all her cardiac symptoms also vanished; and, at her discharge, on the 26th of March 1873, the only detectable sign of what had originally appeared to be a most serious disease, was a slight blunting of the first sound; all the murmurs had quite disappeared, and even the accentuation of the pulmonary second could no longer be detected. She was so well that she left the Infirmary to go direct to service, and in service she has been ever since. I have during the time that has elapsed had occasion to see this patient frequently, and have always found her heart as it was when she was discharged.

You will see, then, that cardiac dilatation is by no means a hopeless disease; that it is invariably revealed by mitral regurgitation, which, in more severe forms, is associated with tricuspid regurgitation also; that, in its slighter forms, it is readily curable, and is almost invariably cured; and that, even in its more severe forms, it may occasionally be cured, and is always susceptible of considerable amelioration, the only exceptions being those neglected cases associated with notable eccentric hypertrophy, forming what is termed the gouty heart.

In simple chlorotic cases, it is sufficient to improve the quality of the blood to secure a perfect cure. For this, as every one knows, there is no remedy equal to iron; and though small doses of ferruginous tonics, especially if continued for a long time, are frequently sufficient, still, the larger the dose given the more rapidly the cure is obtained; and I perfectly agree with Niemeyer, that there is no combination in which iron may be more freely administered than as Blaud's pills—two grains and a half of sulphate of iron, and the same quantity of carbonate of potass, made into one pill with mucilage. Three such pills are to be taken three times a day, and the dose is to be increased by one pill each day till five pills are taken three times a day. The remedy to be continued for six weeks from its commencement, when the cure will be complete. This remedy, notwithstanding the large amount of iron given, does not usually constipate the bowels. When the dilatation is more marked, the tonic action of digitalis on the heart will be also required. Iron, with or without arsenic, may still be given in smaller doses, or may, in certain circumstances, be withheld, but digitalis is paramountly required. Never in large doses, for here we do not desire any immediate contractile effect on the cardiac muscle, but in small doses, never over ten minims twice or thrice a day, because it is only a slowly developed persistent tonic action that we desire, and that is most easily attained without risk by giving these small doses for a length of time. To show how long these doses may be continued without risk, and how beneficial is their action, even in apparently untoward circumstances, I may mention that recently I saw an old gentleman, of over 80 years of age, who has been taking these doses of digitalis for more than four years regularly. When first seen, his heart was so weak that its action was almost imperceptible; he had a feeble pulse, and was subject to frequent fainting fits. Now, he rarely has an attack of this nature, has a good pulse, and a firm, forcible apex beat. Of course, good nourishing food must be given, fresh country air recommended, and perfect rest, or at least the avoidance of all but the most moderate exertion. Stimulants in younger patients are best avoided; in elderly patients they are often useful, and occasionally necessary; but must be used with caution, as simple tonics, and never as goads to exertion, otherwise they do an incalculable amount of harm.

Part Second.

REVIEWS.

Essentials of the Principles and Practice of Medicine. By Henry Hartshorne, A.M., M.D. Philadelphia: H. C. Lea: 1874. Fourth Edition. 8vo, pp. 548.

Since we last reviewed this work, at p. 450 of our number for November 1869, it has passed from its first to its fourth edition. This is no proof of its intrinsic value, though it may be accepted as showing, first, how small an effect honest criticism has upon the sale of a work; and, second, what a trifling modicum of even approximate accuracy passes current in medicine even in these days of advanced science. We have looked up the two or three passages we then referred to as samples of the general inaccuracy and slip-shoddiness of the work, and find that they stand word for word as formerly. There is therefore no reason to change our opinion. The work is enlarged by 130 pages, but it is not improved. The meagreness of its description of the various diseases, the imperfect character of the differential diagnosis, and the harsh baldness, to say the least of it, of its therapeutical portion, render it a very unsuitable work for the medical student, whether tyro or advanced. The woodcuts are no improvement; some of them are positively misleading, *vide* that at p. 385, entitled Ulceration of Peyer's patches, which has no resemblance either to one or more of Peyer's patches, and in no respect depicts ulceration. If there is one thing more than another that requires rectification in medicine, it is the promulgation of such meagre, bald, imperfect, and frequently inaccurate, statements as are to be found in works like the present, as the records of scientific medicine; and, what is worse, their general acceptance by the medical press as such.

The Principles and Practice of Veterinary Medicine. By William Williams, M.R.C.V.S., F.R.S.E. Edinburgh: Maclachlan and Stewart: 1874. Pp. 704.

This important work is one to be highly commended. We have only to compare it with the late Professor Dick's "Outlines," or with Blaine's "Veterinary Art," to see the great progress that has been of late years made in veterinary medicine. Professor Williams is a man who deserves well of his country; for, like the

Israelites of old, much of our trade has been "about cattle," and the prevention and treatment of cattle diseases are matters involving a large amount of money—not to speak of human health and comfort—from year to year. The author, in his interesting account of the rinderpest, does not, we observe, make mention of our townsman Dr Smart's valuable report on that disease as observed in Edinburgh, but, on the other hand, he pays a well-merited tribute of respect to the accuracy of Professor John Gamgee, who, eight years before its importation, vainly endeavoured to rouse the Government to a sense of the impending danger—an apathy which eventually cost the country many thousand pounds, ruined many innocent members of society, and irreparably broke up and destroyed many valuable herds. While speaking in terms of general commendation of the work, we cannot but observe that its value is greatly lessened by an impotent endeavour to hang on to human medicine. Freely acknowledging that the "noblest study of mankind is man," we yet think that a wider field, and one in which greater accuracy is attainable, lies before veterinary than before human medicine; and, in the interests of general medical science, we hope we may ere long obtain from Professor Williams a work which, though more restricted in its scope, will yet be more exclusively his own.

Transactions of the Clinical Society of London. Vol. VII. London:
Longman, Green, and Co.: 1874. 8vo. Pp. 189.

THESE Transactions commence with an admirable address by the President, Dr Prescott Hewett, upon pyæmia in private practice, followed by a no less admirable discussion involving the question of pyæmia generally, and of the influence of "hospitalism" on its production. In the course of this discussion, it came out that the word "hospitalism" was employed to signify two different things: first, simply any hospital—the meaning employed by Sir James Simpson, who thought that the larger a ward, and the older an hospital, the more infectious it was—the only remedy for which was to erect instead of comfortable but dangerous buildings of stone and lime, uncomfortable but presumably healthy movable huts of wood or iron; and, second, the overcrowding and aggregation of many bad cases in one ward—this being simply a case of maladministration, the result of the carelessness of the surgeon in charge. The result attained seems to be the certainty that pyæmia spares no subject and no place, that it is to be met with in palatial homes, in poverty-stricken huts, or in the open field; and if it makes itself more obtrusive occasionally in hospitals than elsewhere, it is simply because the neglect of hygienic regulations is more quickly and more intensively felt there than elsewhere. We are sure that in Edinburgh these conclusions will be readily accepted; and though some of us may still doubt the influence of antiseptics *per se* in preventing pyæmia, none of us can refuse to acknowledge the importance of the careful attention to general hygienic principles with which their use is accompanied.

The other papers in this volume are of varying importance, but all are of considerable interest. Dr Theodore Williams in a series of cases propounds chloral as a remedy for asthma; but we conceive that he would have done better service to medicine had he shown that its chief, if not its sole action, is to cut short the fit, which we believe it will never fail to do in efficient doses. As a cure for asthma, it is no more reliable than any other drug, less so than either iodide of potassium or arsenic. Sir William Gull's paper on Anorexia Hysterica, is a very interesting account of a somewhat rare affection, characterized by extreme emaciation and entire absence of appetite, simulating latent tuberculosis. These are very curious cases, and we can fully confirm Sir William Gull's statement that moral influence and careful nursing are all that is required. We can also recommend Dr George Johnson's cases of temporary albuminuria, the result of cold bathing; and Dr Buzzard's case of extensive paralysis, with recovery under antisyphilitic treatment, as being full of clinical importance. While Dr George Johnson's cases of poisoning by homœopathic concentrated solution of camphor are of great value to the public, in these days when homœopathic solutions are so freely employed under the belief of their harmlessness; the public being apparently quite unconscious that homœopathy has entered on a new phase, and has in too many instances passed from the ludicrous extreme of infinitesimal dilutions to the very opposite—the employment of the greatest possible concentrations of active and poisonous drugs.

A Practical Treatise on Eczema. By Dr M'CALL ANDERSON. Third Edition. London: J. and A. Churchill: 1874. 8vo, pp. 208.

A CAREFUL study of the various forms of eczema, as seen in large general or hospital practice, with the histories of their development and progress, is sufficient of itself, we think, to convince the observer of the truthfulness of the conception of its nature, first clearly enunciated by the great dermatologist of Vienna, and which has found so able an expositor in the author of the present work. The first three chapters contain a very full, and yet clear and concise, account of the symptoms and pathology of eczema in all its varieties, from its commencement in erythema to its squamous termination, and are specially instructive, not only in teaching what eczema essentially is, but also in enabling us to recognise the fundamental disease under whatever form it may present itself. The large experience of Dr Anderson in the treatment this form of disease is seen to great advantage in the chapters treatment, which are copiously illustrated by cases and formul though he is perhaps not cautious enough in his statements as t the use of some of the more powerful local remedies, which, whe incautiously employed, frequently produce very considerable aggravation of the disease. A work like this, which has already reached a third edition, needs no recommendation from us; we would only suggest as an improvement for subsequent editions, that the same powers of generalization which have been so successfully applied to the pathology of the disease should in future be extended to its etiology and treatment.

Free Phosphorus in Medicine, with Special Reference to its Use in Neuralgia. By J. ASHBURTON THOMPSON, Esq. London: H. K. Lewis: 1874. 8vo, pp. 275.

THE use of free phosphorus in medicine has of late been exciting a good deal of attention, not merely in regard to its utility in the relief of neuralgic pain, but also as a most powerful stimulant in cases of extreme nervous exhaustion, from whatever cause that may arise. Under these circumstances the publication of this work is certainly well timed. It contains a full account of the pharmacy of this drug, and of the various formulæ in which it may be administered internally, a short notice of its physiological and toxic effects, and a much longer account of its therapeutic uses; and we know of no other place in which the information regarding the history, pharmacy, and uses of phosphorus is to be obtained in so complete and handy a form as in this volume. The bibliography at the end is alphabetically arranged, and therefore, we think, not so likely to be useful as if it were arranged chronologically. We observe, also, that there is no notice of the valuable observations of Huss on the use of phosphorus in typhus; and though the author has recorded the abstract of Dr George Balfour's paper in the *Lancet* for 1846, he has omitted all reference to the original place of publication, in the *Northern Journal of Medicine* for the same year (vol. iv., p. 284), where is to be found the first full account of phosphor-necrosis in the English language.

Virchow's Archiv, Ausgegeben am 15 Februar. "*Ueber die Circumscripte Keratitis.*" Von A. BOETTCHER in Dorpat.

IN the January number of this Journal we attempted, in connexion with a late paper by Cohnheim, entitled "Noch Einmal die Keratitis," to give a brief account of the controversy at present being carried on in Germany, regarding the inflammatory proliferation of connective tissue corpuscles, and more especially regarding the presence or absence of such proliferation in the cornea, when keratitis has been artificially excited. It was then shown that, with regard to some forms of keratitis, all observers seem to be agreed that the lymph cells are for the most part, if not altogether, of migratory origin, proceeding from the conjunctival vessels at the corneal margin, and moving inwards towards the centre. In such forms of keratitis, as might be expected, the accompanying opacity is centripetal, beginning at the margin and spreading inwards; not so, however, in another form, namely, the circumscribed or central keratitis, which can be artificially produced by slightly irritating the centre of the cornea with a point of lunar caustic or chloride of zinc. Here the opacity is altogether central, the margin remaining quite natural. Now, it will be remembered, that to such cases the attention of the contending observers has been specially attracted, and that Prof. Boettcher of Dorpat in particular professed to have satisfied himself that in them there is clear evidence of the local formation of lymph cells by the proliferation of the stellate connective tissue corpuscles. Cohnheim, on the other hand, in the paper above alluded to, reasserted the opinion that he had expressed in a former work—namely, that in every instance the epithelial layers have been destroyed and cast off at the injured spot, and that through the chasm thus formed the leucocytes of the conjunctival secretion are admitted into the corneal tissue, and there produce the opacity.

It is to the determination of this question regarding the operation of the conjunctival secretion, that Prof. Boettcher's present paper is directed; and it must be said that Cohnheim's theory, which on the face of it looked rather improbable, is met in the present instance by most damaging evidence. The author records eight different experiments, all of them more or less effective. Among them perhaps the best is No. 3, which consists in the entire removal of the membrana nictitans from the frog's eye, and the establishment of a central keratitis in the then constantly exposed cornea. The frogs thus operated upon were placed in water, and it was found that, apparently to protect the irritated eye from the light, they almost constantly kept their heads under the surface. In such circumstances, there was scarcely any possibility of conjunctival secretion finding its way into the wound, and yet the lymph cells collected about the irritated spot in exactly the same manner and degree as they had done in the previous experiments. Another excellent experiment, No. 4, consists in the complete removal of the cornea from a dead frog, and the placing of it, after its surface had been wounded or perforated, under the membrana nictitans of a living one. If Cohnheim's theory were correct there would be, in such a case, the certain entrance of a large number of leucocytes into the dead cornea through the chasm in its surface, since the whole conjunctiva is irritated by the presence of the de matter, and its secretion in consequence is richly loaded wi leucocytes. The fact, however, was not so, for no such collecti was found either at the margin of the wound, or in the tiss immediately surrounding it, and there was an entire absence o those "spear and needle figures," radiating from the wound, whic Cohnheim regards as conclusive evidence of penetration by th conjunctival secretions.

Prof. Boettcher in this paper refers at some length to a recen investigation of this subject by Prof. Eberth of Zurich (Untersuchungen aus dem pathologischen Institute zu Zurich, 1874), in which the author entirely rejects the conjunctival origin of the corpuscles, and further differs from Cohnheim as regards the manner in which they first appear at the irritated spot, asserting, in agreement with Boettcher, that they first appear at the periphery of the irritated spot, and not about the margins of a central opening in the epithelial layer, as Cohnheim would have it. Eberth, however, holds that even in central keratitis the cells are of migratory origin, and come from the vessels of the corneal margin; and here he differs from both Cohnheim and Boettcher. This point may perhaps require further investigation, but in the meantime it may be noted, that by this last paper of Prof. Boettcher, Cohnheim's theory, that in central keratitis the cells come from the conjunctival secretion, seems to be severely damaged, if not completely demolished.

On Spermatorrhœa: Its Pathology, Results, and Complications. By J. L. MILTON, Surgeon to St John's Hospital for Diseases of the Skin. Tenth Edition. Pp. 166. London: R. Hardwicke: 1875.

A WORK which has reached its tenth edition is almost independent of criticism; still, we must tell Mr Milton that there is much both in the manner and the matter of this book with which we cannot agree.

It is a difficult subject to handle, and in some respects it is well and carefully done. The treatment, especially, is founded on common sense in many important particulars, and he is right in urging, that cases such as he describes should neither be made too little of, nor left to advertising quacks. But here and there the language used is both unprofessional and ungentlemanly. Sir Everard Home is called a "wretched villain," p. 5, and Rousseau a "filthy wretch." Other specimens nearly as strong could be culled with ease from these sparkling pages. But strong statements find their maximum in the following sentence, in which it is difficult to say whether the absurdity or the mischievous teaching is most prominent:—"A man who remains continent can hardly reach twenty-six (years of age) without becoming partially, if not wholly, impotent. At twenty-five there is a risk, and I am not quite certain that he is safe at twenty-four." (P. 57.)

Truly, if Mr Milton's disciples are numerous, we cannot wonder at his large experience in the treatment of gonorrhœa and syphilis.

The Army Medical Service in the Past and Future: An Expostulation. By A. M. D. London: J. and A. Churchill.

THE Secretary of State for War lately excused himself for not taking steps to ameliorate the condition of the Army Medical Department, by saying that there was a difficulty in ascertaining what its members really wanted. Let him "read, mark, learn, and inwardly digest" this pamphlet, and act on its suggestions, and he will unquestionably do that which is best, both for the State and the Medical Officer. It is an excellent and true statement of the case, from the pen of one who has evidently a thorough knowledge of the subject, and the best interests of the service at heart. It is difficult to understand how, after perusing it, any one can advocate, on other than purely selfish grounds, a return to the old regimental system.

Part Third.

MEETINGS OF SOCIETIES

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING VI.

Wednesday, 7th April 1875.—Dr WATSON, *Vice-President, in the Chair.*

EXHIBITION OF PATIENTS.

I. *Dr Grainger Stewart* introduced a GIRL aged ten, suffering from chorea. She was able to walk about the room, and exhibited comparatively little choreic movement. Dr Stewart stated that four days before, the chorea had been so violent that she could neither walk nor stand, and could not feed herself or perform any voluntary movement. This state of matters had existed for a week or more before her admission. The rapid improvement was due to a method of treatment recommended in such cases by Dr Hammond, in his recently published clinical lectures. It consisted in the application of ether spray along the spine for about five minutes each day. In the patient now before them, this had been don twice a day for four days, with the most marked benefit and rapi improvement that Dr Stewart had ever witnessed. He wished t express no opinion as to the rationale of the treatment, and woul only add that the suggestion was not original on Hammond's part, but ascribed by him to a German author.

II. *Mr Annandale* showed a MAN whose right carotid artery he had tied, in order to check the progress of an aortic aneurism, which was projecting into the neck, pressing aside the trachea, and causing great pain in the head, neck, and shoulders. The operation had been followed by a complete relief to all the symptoms. There was still pulsation in the aneurism, but this was very slight compared to what it was before the operation, and the aneurism itself was decidedly diminished in size.

III. *Mr Bell* showed a LITTLE GIRL, whose case illustrated some points in the paper Dr Macdougall read before them at their last meeting. She had been admitted to his ward, suffering from acute necrosis of the ulna. He had removed the whole shaft of this bone, leaving, however, the periosteum. Although previously she had been hectic and almost pyæmic, the result had been rapid recovery and complete restoration of bone. The movements of the forearm were perfect.

EXHIBITION OF PATHOLOGICAL SPECIMENS.

IV. *Mr Annandale* showed—(1.) A TUMOUR OF THE UPPER JAW, which he had removed by excision. The patient, before the operation, suffered from ptosis, protrusion of the eyeball, deafness, and partial paralysis of the face on the affected side. The diagnosis of the case was much assisted by a careful examination of the eye with the ophthalmoscope, made by Dr Argyll Robertson, in order to determine the involvement or non-involvement of the brain. The result of this examination was to determine that the brain was not implicated. Consequently, the operation was performed, and the removal of the jaw and tumour had entirely relieved all the symptoms.

(2.) An example of EPITHELIAL CANCER OF THE HAND, removed by amputation through the forearm. The patient was a man seventy-four years of age, and was making an excellent recovery.

(3.) An example of a SOFT FUNGATING CANCEROUS TUMOUR growing from the head of the tibia, removed by amputation of the thigh.

(4.) A FOOT illustrating the results of an excision of the os calcis.

(5.) An OBLIQUE SECTION OF THE ARTICULAR END OF THE FEMUR, removed by operation, in order to relieve an aggravated case of "in-knee," which had resisted mechanical treatment. As far as Mr Annandale was aware, the operation was a new one, and nsisted in making a free lateral incision along the inner aspect of he knee-joint, opening into the joint, dividing the lateral and crucial ligaments, and sawing off an oblique portion of the condyles of the femur, without interference with the patella or its ligament The articular surface of the tibia was not interfered with.

(6.) A preparation illustrating disease of the HEAD OF THE HUMERUS, for which he had performed excision with a good result.

(7.) The parts removed by excision, in a case of DISEASED KNEE-JOINT.

V. *Mr Bell* showed an ARM which he had removed from a female patient, at the shoulder-joint. The whole limb had been hypertrophied, much distorted, and perfectly useless. The disease seemed to him to be elephantiasis with symptoms of anæsthetic leprosy. The curious points in the case were, that the woman had never been out of Scotland, and that there was no history of syphilis. Owing to the inconvenience given to the patient, and the hopelessness of any local or constitutional treatment, he had deemed its removal necessary.

Photographs of the patient were also shown.

VI. *Dr Grainger Stewart* showed the HEART AND PERICARDIUM of a man whose history was the following:—J. S., æt. 25, ironmonger, Edinburgh. Admitted to Ward VII., 22d March. *Complains of* dyspnœa and swelling of legs and abdomen. Been ill about three weeks. Patient has always been a healthy man till about the middle of December last, when he had an attack of rheumatic fever, for which he was treated in the Infirmary. He does not seem to have made a satisfactory recovery from this illness. *Present illness* commenced about three weeks ago, in the form of a pneumonic attack in the left chest, from which more or less he was still suffering when he entered the Infirmary. *On admission.*—Besides the pneumonia above mentioned, patient was suffering from effusion into the pericardium to a considerable extent. There was bulging in the precordial region, and an area of absolute dulness over the position of the heart, the vertical diameter of which was 5 inches, and the transverse at the nipple level 7 inches. The heart sounds were dull and indistinct, and so completely muffled, that it was impossible to distinguish whether they were pure or otherwise. The pulse was rapid, full, and jerking, but speedily collapsing under the finger. The man's general appearance was that of extreme distress. He had orthopnœa, his breathimg was rapid and gasping. His face was pale and covered with perspiration, and he suffered from paroxysms of spasmodic cough which greatly exhausted and pained him. There was also some amount of ascites. He was treated for four days in the usual way by means of expectorant stimulants, and the free use of counter-irritation to chest, by means of mustard and fly-blisters, and the house-physician, Dr Cash, was directed to tap the pericardium in the event of more aggravated symptoms occurring. On the morning of the 26th, the patient seemed a little easier, but on awakening from sleep syncope suddenly came on. He fell back, and was to all appearance nearly gone. In these circumstances, Dr Cash introduced a fine aspirator needle into the pericardium in the fourth intercostal space close to the sternum on the left side, and withdrew 14 ounces of fluid. He was much relieved for a time, but the benefit was only transitory, as he sank about two hours later. The fluid withdrawn was serum containing much pus, some blood-cells, and a large quantity of lymph in flaky shreds. *Sectio* (twenty-four hours after death).—Body well nourished. A few purpuric spots in parts, chiefly over abdomen. *Thorax.*—The pericardium was thickened and vascular. Its surfaces were coated with lymph, and a few ounces of fluid were contained in its cavity. *Heart.*—Heart's muscular substance was dark-coloured, soft, and friable. The microscope revealed the granular appearance characteristic of myocarditis. The cusps of the mitral valve were thickened. Every segment of the aortic valves was diseased. The corpora arantii were thickened. The valves were eroded and torn. At the base of one of them the substance of the septum ventriculorum was eaten into and expanded into a small aneurismal pouch. The valves were quite incompetent. *Lungs* were greatly congested, the middle portion of the left was pneumonic. *Abdomen.*—*Liver* was fatty, and weighed over 6 lbs. *Kidneys and Spleen* enlarged and congested.

VII. *Dr J. Dunsmure, jun.*, showed PART OF A BRAIN OF A BOY, seven years of age, who had chronic hydrocephalus, and who died after forty-eight hours' illness from scarlet fever. Dr D. gave the following history of the case:—The boy, when two years of age, had a discharge from his left ear, which ran for a short time, and then dried up. It commenced again two years and a half ago, and has continued ever since. At the same time his mother noticed that he became dull, and his memory began to fail. Previously, he had been particularly intelligent for his age. Shortly after, his speech became affected, and she observed his head was becoming broader across the vertex. These conditions gradually became worse, till about twelve months ago, when his gait became impaired, and she saw that he stooped rather, and dragged his limbs when walking. Three months ago, when he walked, he leant over to the right side, and shortly after lost the power of walking altogether. During the last year his head had increased considerably in size, and he often awoke in the night complaining of pain in his head, and that he felt giddy, which symptoms his mother was in the habit of relieving by compressing the head firmly in her hands. He never had a convulsion. In the last three months he experienced considerable difficulty in passing water, and was often a quarter of an hour before he managed to void it; at other times, it came away involuntarily; and his bowels became much confined. He had a mild tack of hooping-cough three and a half years ago. His maternal grandmother died of consumption, besides some other relations on both father's and mother's side.

The boy was well nourished, and his muscular development good for his age. The forehead projected considerably anteriorly, and the vertex was very broad posteriorly, giving the top of the head a triangular form, with the apex in front. There was marked bulging in the occipital region. The circumference of the head was 22 nches, the measurement from one mastoid process to the other (over the head) was 12½ inches, and from the root of the nose to the base of the occipital bone 15 inches. Ossification was complete, and there was no trace of the fontanelle. His speech was greatly impaired; and on his answering a question, he said the first word or two correctly, and then uttered merely inarticulate sounds. On supporting him, and making him walk, he stooped much, and threw his legs forward, quite like a patient suffering from locomotor ataxia. When lying in bed, and on asking him to bring his legs together, he invariably brought the one leg over the other, and then had to replace it, showing the power of co-ordination was affected. The same thing happened on asking him to clap his hands. He unfortunately took scarlet fever, and died in forty-eight hours.

The post-mortem was made twenty-four hours after death, and on removing the skull-cap, which was thinner than natural, the dura mater was found very adherent. The brain was sent to Dr Blair Cunynghame for examination, who favoured me with the following account of it:—The brain appeared much larger than natural, and somewhat soft. The convolutions were flattened, and the sulci obliterated. The tissues at the base were also soft and flattened. The corpora albicantia were separated, and appeared as if they were stretched. The left side of the cerebellum was about twice the size of the right, and its outer two-thirds presented a swollen appearance, with a very smooth surface and rather flattened, which, upon examination, proved to be a tumour about the size of a bantam's egg. At the outer side there was a jelly-looking bit about the size of a large filbert, and which, upon being punctured, allowed a sero-purulent-looking fluid to escape, and which, under the microscope, consisted of cells varying in form, circular, and tailed, with a considerable quantity of granular fatty debris, many of the cells being distinctly nucleated. A scraping from the tumour showed round and irregular-shaped nuclei in great quantity, with cells about the size of pus corpuscles and slight fibrous stroma. The substance of the tumour was very dense and firm. Dr Cunynghame was of opinion that it was of a tubercular nature. The lateral ventricles were very much enlarged, and when the brain was first removed from the body, two and a half pints of fluid escaped. The brain weighed 3 lbs. 14 oz. All the other organs were healthy.

The case appears to me to be one of interest, as I think, from its history, it cannot be doubted that it is an example of a dropsy of the brain, caused by an obstruction to the venous circulation from the presence of the tumour in the cerebellum, as the presence of tumours is generally the cause of chronic hydrocephalus when the disease has not been congenital.

VIII. *Dr Watson* showed—(1.) The REMAINS OF THE NECK AND HEAD OF FEMUR, which he had excised for disease. In the early stage of the case an abscess had formed within the pelvis, which had pointed to the front of the thigh, and been evacuated there by another surgeon. During the operation of removal there was some difficulty in reaching the head of the bone, and it was ultimately found inside the pelvis. The patient had done well.

(2.) The ASTRAGALUS AND EXTREMITY OF THE TIBIA, removed for disease by a single T-shaped external excision.

(3.) The CARPUS AND METACARPUS removed by a single dorsal linear incision.

(4.) PART OF KNEE-JOINT excised for disease.

(5.) The ARTICULAR SURFACES OF THE KNEE-JOINT removed in a young patient, by a new method. When excision of the knee-joint is performed in children in the usual way, there is a great risk of stoppage of the growth of the limb, owing to removal of the epiphysis. He had therefore in this case and also another one, by means of a very fine saw, resected the cartilaginous surface of the femur by a curvilinear section, without removing the epiphysis.

(6.) TWO CALCULI removed successfully from children.

(7.) THREE CALCULI removed from a patient through perineal sinuses. Some years previously, he had suffered from extravasations of urine due to injury, for which his perineum had been incised in the Dreadnought hospital. Perineal fistulæ had resulted. These, Lizars of Edinburgh had attempted to close, but without success. The patient had then come under his own notice for retention of urine. On passing in a probe through the perineal sinuses, the calculi shown were detected and removed. A grooved staff was then passed along the urethra, to ascertain if anything could be done to restore its patency. This, however, was found impossible, as it was obliterated for about three inches. The most curious feature of the whole case was, that the paternity of a child had been fixed on him, and in a final appeal to the Court of Session it had been decided that he was the father. Now, how this could be the case, when he took into consideration the state of the man's urethra, Dr Watson was unable to explain; but perhaps some of the obstetrical members could throw light on the subject.

IX. *Dr J. W. Begbie* then read a paper on HÆMATINURIA, which appears at p. 1005 of the present Number of this Journal.

Dr Watson remarked, that the members must not only have been greatly interested in the paper read by Dr Begbie, but also in s reappearance among them.

Dr Grainger Stewart did not propose to criticise the views xpressed in Dr Begbie's interesting paper, but was desirous of entioning that the few cases of the disease he had met with closely orresponded to the description given by Dr Begbie. The relation-hip to hepatic affection was well marked. In the last case he had een, the patient had been for some time an out-patient of the nfirmary, suffering from chronic catarrh of the stomach and uodenum with congestion of the liver, and as ordinary treatment id not cure this, he had been brought into the Infirmary. When here, an attack of paroxysmal hæmatinuria had come on. On the rst day of the attack he had albumen in his urine, and this was hought at first to be an example of paroxysmal albuminuria, but n the following day the hæmatinuria appeared. He had seen several xamples of paroxysmal albuminuria, and had found it as common n women as in men. As to the treatment by means of chloride of mmonium indicated by Dr Begbie, he did not recollect having sed it himself, but he would in such cases certainly act on the uggestion thrown out. He thought they were all indebted to Dr Begbie for his communication.

Dr Smart thought the paper they had just listened to with so uch pleasure one of unusual interest. Hæmatinuria, as described y Dr Begbie, suggests that the disorder may have some important athological relations to glycosuria, chylous urine, and albuminuria, ll being characterized by an abnormal condition of the blood, the orbid elements of each being alike eliminated by the same organs. Dr Begbie is disposed to believe that the hæmatinurial condition of he blood is more closely connected with a disordered condition of the iver than with that of any other organ; and this view is well sup-orted by the prominently hepatic symptoms shown to have existed n both patients lately under his care. The microscopic condition f the blood in hæmatinuria (Dr Begbie having been the first to icroscopically examine the blood in this disease) shows it to be n a condition of leucocytosis. If we connect this fact with the aroxysmal character of the malady, and the peculiar leucocythæmic allor of many of those who suffer from it, he (Dr Smart) ventured o think that the disease might in many cases have a splenic origin; nd this opinion receives additional confirmation by the malady eing in many instances quickly and favourably amenable to large oses of quinine. Speaking of Dr Laycock's valuable suggestion s to the neurotic nature of the lesion, we know that shock (rigor) o the trophic system of nerves is followed, less or more, by unctional derangement of the entire glandular system, leading to isordered secretions and blood dyscrasia, the morbid elements of hich have to undergo elimination. But the question naturally rises, Why are the renal organs the emunctories in this disease? hat there is an excess of urea in hæmatinuria, as shown by Dr Affleck's examination, sufficiently explains why this, as also other bnormal constituents, should in this manner be eliminated. This oes not, however, imply that the kidneys are themselves implicated in the disease, although after a time they are liable to become so, as is shown by the presence of tube-casts and other morbid elements sometimes appearing at a later stage of the malady. Again, why is it that the blood-cells are not present in hæmatinuric urine? It is supposed that it is only the colouring matter—the hæmatin—that is thrown off. But we must pause before admitting this conclusion, when we remember that the blood-cells are destroyed by the action of the kidney in the course of their being eliminated, and because albumen is always present in hæmatinuric urine. It is not yet ascertained, however, as Dr Begbie has shown, that the albumen and hæmatin are present in the same proportion in the urine of a hæmatinuric person as they exist in the blood. It is still, however, a fact that hæmatin and albumen coexist in hæmatinuria, and we so far are entitled to believe, until there is proof to the contrary, that they exist there as eliminated blood, the cells being destroyed in the process. This points to a means of diagnosis which might be useful in determining the source of the hæmorrhage when blood is found in the urine. The absence of blood-cells from the blood-stained urine will show that it is eliminated by the kidneys, these organs being in a state of comparative health; while the presence of blood-cells will indicate that the blood is not an excretion of the kidneys, but arises from other structures between those organs and the urethra, even from the non-secreting structures of the kidneys themselves. It is unnecessary to add, after hearing Dr Begbie's paper, that he has given us another very valuable contribution to an interesting class of diseases, having presented the subject in its most interesting and advanced aspects.

Dr Clouston had noticed, in a patient suffering from circular neurosis (*folie circulaire*), being at one time excited, then depressed, that there was one stage where he became thin, and passed bloody urine. He did not at the time investigate this particularly; but he had now no doubt, after hearing Dr Begbie's paper, that it was a case of hæmatinuria of neurotic origin, from affection of the trophic nerves of the kidney.

Dr Gillespie said that they had heard a most interesting paper from Dr Begbie on a very uncommon disease; that the medical view of the cause of the disease, judging by what had fallen from the preceding speakers, seemed to be, cold affecting the trophic centres; but in the case of Sir William Gull, referred to by Dr Begbie, a surgical aspect would also require to be taken, as hæmatinuria was the direct result of injury. In the urine of that patient, broken-up blood-discs were observed, and Dr Gillespie would be glad to hear from his medical friends how they accounted for the hæmatinuria.

Dr Begbie presumed that Dr Gillespie did not mean to question the facts of Sir William Gull's case. He had alluded to that case in connexion with the occurrence of lumbar pain as one of the symptoms of hæmatinuria. In his own cases, and those of others, there was no mention of pain in the loins as a symptom; and, in these, the altered condition of the urine was the only indication of renal disorder. Further, when he read a statement made by a reliable authority, he took it as he found it, without attempting any explanation of the facts. He had been gratified with the reception his paper had met; and he only wished further to remark, in reference to what Dr Watson had said, that he was never absent from any meeting of the Society unless detained by pressing engagements, or circumstances beyond his own control.

X. *Mr Annandale* then read a paper on the TREATMENT OF CERTAIN CASES OF STRICTURE OF THE URETHRA BY A COMBINATION OF INTERNAL AND EXTERNAL DIVISION, which will appear in an early Number of the Journal.

Dr Watson believed that the filiform bougie which Mr Annandale had described as an addition to Maisonneuve's instrument, was really part of it. Gouley's plan was to use a great many extremely fine whalebone bougies, by which the false passages were filled, and then one could be got into the bladder, upon which the eye at the end of the metal instrument was threaded, and thus guided it through the contracted part of the canal. As to internal division of stricture by Maisonneuve's instrument, there was no plan on which the vials of wrath had been more poured by a former member of the Society. However that might be, it was certainly the case that it divided the urethra effectually, the only objection being that it divided far more than was necessary, as could easily be seen in employing it in a healthy urethra in the dead subject.

Mr Annandale had divided with this instrument many strictures, and had never seen any bad results, so that whatever amount was cut no harm appeared to follow the proceeding.

Mr Bell alluded to an interesting and acrimonious discussion lately going on between Van Buren and Gouley as to the originator of the whalebone and eyed bougies. The whole subject of the treatment of stricture by division was a fascinating one, and one that had assumed various phases. He would certainly have been a bold man who, twenty years ago, had advocated the internal division of stricture in the Society.

XI. *Mr Bell* then read a paper, entitled, CASE OF OPERATION FOR INTUSSUSCEPTION IN A CHILD, which will appear in a future Number.

Mr Annandale was glad to listen to Mr Bell's paper, as he had already expressed similar opinions as to the treatment of obstruction in the adult. He felt certain that, in such cases, an exploratory incision should be made after other means had failed to afford relief. It did no harm, and gave the patient a last chance.

Dr Watson also held that this should be done. He had now performed such an operation in about eight cases. In some instances he would urge it; but, in others, he would be doubtful. In one case of obstruction for the third time in the same patient, he was recently called in. The coils of the small intestine, distended with air as far down as the caput cæcum coli, could be seen distinctly through the abdominal wall. The obstruction was therefore at the ileo-cæcal valve. He did not operate in this instance; and as he had not heard of the patient's death, he hoped he had recovered without operative interference. In another case, the patient got relief by a spontaneous evacuation of the bowels, but ultimately died from suppuration in a limited peritoneal area. In a third case he operated, but could find no sufficient cause of obstruction. There was a difficulty at the close of the operation in stitching up the abdominal parietes; a suitable dressing was applied, and the patient ultimately succumbed. In the last case he wished to bring before the Society, he had performed lumbar colotomy successfully four years ago for obstruction at the sigmoid flexure. The artificial anus was closed by an indiarubber ball attached to a belt, and it thus adapted itself to any movements on the part of the patient. He was quite aware when the bowels were about to be evacuated, which was generally once in the twenty-four hours. Another indiarubber ball, which had been kept soaking in an antiseptic solution, was applied after the evacuation. By this means all discomfort to the patient was avoided.

Mr Bell pointed out that his paper had been intended to bring forward "abdominal section in cases of obstruction in children." He held that the diagnosis of invagination was comparatively easy when the rectum could be explored. He had carefully studied Mr Annandale's case; and he only regretted that Dr Watson's cases had not been put on record. Mr Hutchinson's case was encouraging; and so was the one he himself had just recorded. He hoped that the results would be still more successful.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIV.—MEETING VI.

Wednesday, 10th March.—Dr Matthews Duncan, *President, in the Chair.*

I. *Professor Simpson* exhibited a FIBROUS POLYPUS which he had removed from the cavity of the uterus by torsion. It had been expelled into the cavity of the cervix uteri, but not entirely, and a constriction indicated where the internal os had grasped it after the expulsion into the cervical cavity. As the os externum was very narrow, he (Dr Simpson) had first divided the cervix at both sides with a pair of strong scissors, and then seized the exposed polypus with a pair of abortion forceps. The narrow pedicle which grew from the back wall of the uterus was easily twisted through, and the patient made a good recovery.

II. *Dr James Young* showed the PLACENTA of a dead-born fœtus. He explained that it exhibited appearances of thrombosis in various cotyledons.

III. *Dr Keiller* showed a CAST OF A FŒTUS, with an interesting and rare malformation of one of the inferior extremities. The right leg, a short way above the ankle-joint, was the seat of a supposed intra-uterine compound fracture, which had healed and united, with some antero-posterior displacement in utero. There was talipes valgus of the left foot. The fœtus was otherwise healthy.

Dr Macdonald had seen a case some years ago where both arms and the left leg were amputated spontaneously in utero.

Dr Cuthbert had seen a fœtus at about the third month, where the process of amputation was going on, the cord being twisted round the limb.

Dr Young had had a case of intra-uterine fracture of the femur, in which, under appropriate treatment, the fracture united perfectly.

IV. *Mr Lawson Tait's* paper on ENLARGEMENT OF THE THYROID BODY IN PREGNANCY was read. (This communication appears at page 993 of this Journal.)

Dr Coghill said he had a child who was born with a goitre, which he attributed to the circumstance that his wife, during pregnancy, was under malarious influences. It disappeared shortly after birth. This fact, he thought, would support Mr Tait's views.

Dr C. Bell was struck by the fact, that in general goitre benefited by the use of iron, but in Mr Tait's case it was hurtful. It would be interesting to know, if in Derbyshire, where goitre is common, it is most so in pregnant women. He had seen a goitre in a pregnant woman, which was quite unaffected by that condition. She was a strong robust-looking woman, accustomed to out-door work. Goitre was occasionally met with in the upper ward of Lanarkshire, but although he had spent much time there, only three other cases had come under his notice. They occurred in the same family, which occupied a low-lying cottage, a good deal shaded by trees. The springs were particularly pure, many of them flowing over limestone.

Dr Macdonald had seen cases of goitre in pregnant women, but was not aware of their alleged special increase in size during pregnancy, and had not paid any attention to the point. He would hardly say that the goitre was caused by the pregnancy, but rather that it was the effect of endemic influences, and was exaggerated by the condition of pregnancy. He was astonished at the alarming effects of the one-third grain injection of ergotin mentioned in the paper, as he was in the habit of using ergotin frequently, and had just recently injected first two grains, and again on the following day three grains, in a case of menorrhagia, with nothing but good resulting.

Dr Simpson thought the paper a very valuable one. He had observed minor degrees of the conditions described in pregnancy, but had not met with any case of permanent increase in size resulting from the pregnancy. He thought there was a rare field for studying the question in Derbyshire and Cumberland. He had had a case where the whole mechanism of the head, in its passage through the pelvis, was deranged by the presence of a goitre in the fœtus. It had disappeared soon after birth. He knew that some authors had noticed small doses of ergot followed by alarming symptoms.

Dr Fehling had had a case under his care of congenital goitre, which did not diminish after birth.

Dr Keiller had seen a case in which congenital goitre had interfered with the birth of the child. The mother in this case came from Cumberland, but had no goitre; the goitre in the child had afterwards almost entirely disappeared. The paper of Mr Tait was unquestionably an interesting one, and showed that its author had looked with a single eye at the character and history of the cases on which he founded his observations. The connexion of the bronchocele with the condition of pregnancy was doubtless very obvious, as stated by Mr Tait, but he (Dr Keiller) could not help thinking, while listening to the illustrations given in the paper, that the goitres and the pregnant condition had been too intimately associated—as if, indeed, the beginning and end of both were interdependent. Dr Keiller had not enjoyed opportunities of observing the thyroid gland under the stimulation of pregnancy in districts in which goitre was common; but from his (Dr Keiller's) observation of cases of goitre occurring in pregnancy and otherwise, he could not agree, that by pregnancy alone the thyroid could be so invariably influenced as described by Mr Tait. In other physiological as well as diseased conditions in the female, apart from pregnancy, and especially in ovarian, uterine, and other cases, Dr Keiller had observed the sympathetic relations between the thyroid and other glandular structures of the uterus. These relations indeed were known and admitted long ago.

Dr M. Duncan knows goitre to be not rare in young women: he sees them sometimes of considerable size, and frequently curable. In connexion with pregnancy, he has had his attention called by women to the enlargement of the gland, and also in menstruation; and he thought the subject ought to be studied. He had attempted to make out the enlargement of other glands, such as the spleen and liver, during pregnancy, in the living subject, but could not convince himself in this way that any such enlargement existed. He was sure it was the opinion of the Society that they were very much indebted to Mr Tait for his able paper, and suggested that the Secretary should inform him of this circumstance.

V. Exhibition of the work on INVERSIO UTERI OF CROSSE by the *President.*

Dr Duncan remarked that when recently studying inversio uteri, e had been amazed that there was not a copy of Crosse's great work n the subject in Edinburgh. He had written to Crosse's son, the resent Dr Crosse of Norwich, who had sent him his own copy of is father's work, and also five additional volumes, which formed a asis for the continuation of the book. It is a very remarkable work, full of learning and ingenuity, and containing many valuable bservations, recorded with distinctness and accuracy. It was reatly to be regretted that Dr Crosse did not live to complete it.

Part Fourth.

SURGICAL PERISCOPE.

EXCISION OF AN OVARIAN CYST FROM A CHILD SEVEN AND A HALF YEARS OLD. By GEORGE CUPPLES, M.D., San Antonio, Texas.—Julia Young, seven and a half years of age, of healthy parents, herself having previously enjoyed good health, but of a nervous temperament, and convalescing from a severe attack of whooping-cough, was brought to me on the 16th of June 1874, to be prescribed for, for a swelling in the belly. The swelling was discovered by the mother three months previously. She (the mother) described it as "a fulness or lump in the left side, which has continued to increase."

The history of the case was not elicited until some time after the operation, and then, accidentally, the mother recollected that three years previously the child, while playing, was knocked down by a heavy piece of timber, which fell on her abdomen and pinioned her to the earth; so heavy was it, that it necessitated the assistance of another woman with herself to raise it from off the child in order to extricate her.

On the date before-mentioned, I elicited the following:—For the past few days the swelling has increased in size; the child complains of cramps in left lower extremity; in fact, so severe are these cramps that she has been awakened from her slumbers by them.

On inspection, I found the superficial veins on left side of abdomen somewhat enlarged, and a tumour, ovoid in shape, movable, and free, occupying a space from three inches above to three inches below the umbilicus in the mesian line. It presents the appearance of a miniature gravid uterus, fluctuation distinct on left side, whilst there is dulness on percussion on opposite side.

My diagnosis is, "cyst of left ovary." I ordered the child to have castor-oil and turpentine. After the action of the purgative, the salient features of the tumour became more distinctly marked.

On the 17th of June, the day following, I introduced an explorin trocar and gave exit to about half an ounce of thick albuminou fluid, clear, but dark. The flow ceased in consequence of the increased viscidity of the fluid obstructing the canula. My diagnosis was now confirmed, and I determined to extirpate the tumour so soon as the patient should be more fully relieved of the pertussis. I therefore ordered her return in fifteen days (the parties live in the country).

On the 23d of July, the patient returned for operation. The tumour was enlarged, and occupied a position regarding the left hypogastrium, having passed the mesian line.

25th July.—The tumour to-day occupies an oblique position between the left hypogastrium and right hypochondrium; the position having changed in the last twenty-four hours. It is tense, tender, and doughy. The child's general health is good; her cough still troublesome at intervals, and at times very severe. I ordered her to have half an ounce of castor-oil, with a view to evacuate the bowels prior to operation, which I set for to-morrow.

26th July.—Aided by Drs A. E. Carothers, of Saltillo, Mexico, F. Paschal, T. R. Chew, and A. Ansell (the latter in charge of the anæsthetic), I proceeded to the excision of the tumour.

I made an incision two and a half inches long, a line or two to the left of the mesian line, severing severally the integument, fascia, aponeurosis, and peritoneum, and exposed the tumour, which now became very prominent. Introducing my finger, I found the tumour free from adhesions. I then extended the incision two and a half inches more—that is to say, one and a half inches upward and one inch downward—making the wound five inches in length. At this moment vomiting set in with such violence as to cause the extrusion of the whole of the small intestines, which were carefully enveloped in flannels dipped in warm water, to which had been added liquor potassæ permangan., and there retained until after the removal of the tumour.

This untimely vomiting was due to the parents having given milk punch instead of a small toddy, which I ordered to be given an hour previous to the operation; this caused a delay of about fifteen minutes.

The tumour was now punctured with a "trocar of Pouteau," and its fluid contents drawn off in such a manner as to effectually prevent any escape into the abdominal cavity. The pedicle being thin, and the condition of the parts apparently favourable for enucleation, an attempt was made to strip off the investing membrane, but it proved to be too delicate to bear entire removal; the pedicle was therefore secured by a single carbolized catgut ligature, divided, and returned within the cavity. With considerable difficulty I returned the intestines, and closed the wound by six deep silver-wire sutures, including in these the peritoneum, and five superficial twisted sutures. The operation was concluded in sixty minutes, during all of which time the little patient was fully anæsthetized by chloroform. The whole quantity of the anæsthetic consumed in this operation was three drachms by weight. The pulse continued good throughout. The wound was covered with a pledget of lint dipped in carbolic oil (1 to 80), over which was applied a pad of linen, and over all an evenly laced flannel bandage.

Immediately after the operation the temperature was 98·4°; pulse 114; skin soft, warm, and moist; no vomiting. Ordered tinct. opii, gtt. x., in brandy and water; to have small pieces of ice.

The composition of the tumour was of the usual character—sebaceous matter, loose hair, in considerable quantity, and extremely fine. There were also a number of small cysts, from the size of a No. 6 shot to that of a good-sized marble. There was also one that had attained the size of a hen-egg. The entire size of the tumour was that of a cocoa-nut. Its weight was fifteen ounces, of which the solid part weighed three and three-quarter ounces.

The case went on favourably and the child recovered—returning home on the fourteenth day.

The rarity of ovarian cyst before puberty is such that I have been able to collect from the limited resources within my reach but nine cases. The case related above makes the tenth.—*Richmond and Louisville Medical Journal*, January 1875.

ARE THE KIDNEYS ACTIVE ELIMINATORS OF ETHER?—A young man, aged about 30, was brought into the hospital for a railroad crush of the forearm, requiring amputation above the elbow. He had been a drinking man, but there was nothing in his appearance to suggest excess in this way, nor was there any other external sign of disease. He was taken into the clinic-room, and etherization was begun. Fortunately, I had my fingers on his pulse, and suddenly felt it drop after but a few inhalations had been taken. The patient seemed to be sinking. Etherization was suspended at once, restoratives were applied, and he was removed from the room. In the afternoon he had reacted, and another attempt was made to operate. We were now fully on guard as to the ether. After a very few whiffs, the pulse began to sink, and we were threatened with a repetition of the morning's experience. The ether was withdrawn, and the operation was performed without it by Dr A. V. Meigs.

For a few days the patient did well as to his stump, and also in other respects; but he then grew gradually worse, became feverish and delirious; the stump ceased to heal, but did not contract as in pyæmia cases. An examination of the urine was unfortunately overlooked, as there was no dropsy, nor other symptom to call attention to it. The condition was not pyæmic, but it was attributed partly to the accident and partly to habits of the patient, and was classed under the general head of surgical fever.

The unfavourable symptoms continued, and death occurred i about two weeks after the injury was received. The post-mort showed the kidneys to be in a marked stage of fatty degeneratio so that their functions must have been very materially interfer with. What relation had the inability to take ether to do wi this condition? May not conditions of this kind often explain th unpleasant and even fatal action of an anæsthetic?

The practical lesson is obvious. Before a serious operation, should there be the slightest reason to suspect hidden trouble, examine the organs and the secretions, and be on guard.

Ether at Night.—Caution as to the use of this agent at night cannot be too frequently urged. The lights should always be *above* the operator and etherizer, and the neighbourhood of a low grate or of an open stove should be carefully avoided. The vapour of ether is very heavy, and falls rapidly, as any one can test by pouring a little of the liquid into a saucer and watching it roll and fall over the sides. One of the most exciting and seriously threatening scenes that I have ever been engaged in happened in this way.

A man required amputation of the arm (which was crushed by an engine) high up. There was, fortunately, just room for the tourniquet. The residents of the hospital, together with Drs Agnew and Herbert Norris, were assisting. The latter administered the ether from a large sponge, and placed the bottle on a chair at his side. I had just removed the dangling fragments of the limb, when an assistant brought a candle close to the stump, and almost directly under the sponge. Instantly we were in a blaze. Dr Norris involuntarily started back, and, in doing so, knocked over the chair with the large ether-bottle, which broke! Its contents were quickly spread, and the whole ring of the amphitheatre was on fire. A nurse lifted the patient from the table and placed him on the floor. Fortunately the tourniquet held. Others sprang into the side wards and seized blankets, coats, and anything at hand, and we soon stamped the fire out, but were all more or less singed. The walls of the amphitheatre bore evidence for a long time, in the scorched wood and blistered paint, of the danger. The exposed parts of the patient were superficially burned, but he never knew what had happened. He was placed on the table, and the operation was finished. The thorough anæsthesia here was fortunate for all. Had the fire taken place earlier, terror and excitement from ether combined would have made the man uncontrollable, and a fearful panic might have occurred in the hospital. As it was, no alarm was spread beyond the room.

Night operations with ether should, therefore, only be those of absolute necessity, and with the cautions enjoined there need be no danger. Chloroform, not being inflammable, has a great advantage over ether in this respect; but, I confess, I am afraid of it.

Maggots.—If you keep the maggot-fly away, the maggots won't come. She deposits her little white masses of living young with astonishing quickness, and they are ready to go about their work with almost equal celerity. It is therefore nearly impossible sometimes to keep them away. But what if they do come? They are fearfully disgusting, that is true; but, except when they get into the wrong places, they do no harm: nay, it is a question whether they don't do a certain amount of good by acting as scavengers. They consume only the dead matter. I have often seen a wound healing kindly under them as though under a living poultice—a poultice of active properties, converting harmful dead material into live, harmless substance.

Now, I am no advocate of maggots: as I said, they are disgusting; but, if they do come, get rid of them quietly, and do not make so much fuss about them, and so favour the popular notion that the man is going to die because he is breeding worms.—*Philadelphia Medical Times.*

Dr Anger, at the Hospital Saint Antoine, records an excellent case of suture of tendons, six months after they had been divided. An adult had had both the extensor tendons of the little finger completely divided. Extension was lost, and the finger was permanently flexed in the palm. An incision was made in the dorsal aspect of the hand. The ends of the tendons were separated about two inches and a half. After a careful dissection and considerable traction, the ends were approximated by silver sutures to within an inch of each other, and the finger was kept extended. In the end union took place, and the use of the finger was regained.

The author refers to similar cases by Syme and Sedillot, and at some length to one by Chassaignac.—*Gazette des Hôpitaux*, 1875, p. 195.

Immediate Transfusion.—Dr Chadwick of Boston reports a case of immediate transfusion for extreme anæmia, the result of repeated hæmorrhage after delivery. Aveling's instrument was used, and the blood was obtained from the arm of Dr C. P. Putnam of Boston. Eleven ounces of blood was transfused—a larger quantity than was intended, owing to a mistake in the making the bulb of the instrument, which it was found held four drachms instead of two, as was supposed.

Little change for the better resulted from the operation; after severe vomiting, blood to the extent of four or five ounces was lost from the wound, and she died in about thirty-six hours after operation. At the autopsy fatty degeneration of the heart was found, and extreme anæmia of all the organs of the body, except the spleen.

It was worthy of note, that the *anæmic souffle* which existed prior to the operation was not in the least altered by the large quantity of blood injected.—*Boston Medical and Surgical Journal*, 14th January.

Distal Ligature of Carotid and Subclavian Arteries for a Case of Innominate Aneurism. By Frederick Ensor, M.R.C.S.—Patient, a Kaffir-Hottentot æt. 50, had evidence of a dilated arch of aorta; with a second pouch or dilatation of the innominate artery, the symptoms of which were getting daily worse. On 8th September the carotid was tied just at omohyoid with a silk ligature, and immediately afterwards the third part of right subclavian, also with silk ligature. No bad symptoms followed. On the 21st day the subclavian ligature separated, on the 29th the carotid one. By the 44th day wounds were healed, and patient's state was improved in many respects, specially as to pain and difficulty in respiration and deglutition.—*Lancet*, 6th February.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario College of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

The Effects of Alcohol.—The vexed question as to the value of alcohol as a medicine and as a food has of late been the subject of consideration by several eminent writers. The first number of the *New York Psychological and Medico-Legal Journal* contains a communication from Dr William Hammond on this subject. He undertook a series of experiments on himself to ascertain the influence of alcohol, when, 1*st*, the food taken was sufficient for the wants of the organism; 2*d*, when it was insufficient; and, 3*d*, when it was more than sufficient. The inferences which he draws are as follows:—When the supply of food is abundant, there are no special circumstances existing which render the use of alcohol advisable. When the quantity of food is not such as to allow the due performance of such physical or mental labour as may be necessary, or when the digestive or assimilative functions are not so efficiently performed as to cause the digestion and appropriation of a sufficient quantity of food to meet the requirements of the system, then the proper use of alcohol is to be commended. In the third series of experiments, in which more food was taken than was necessary, the ill effects of alcohol were well marked. Dr Hammond advances the following hypothesis:—"It is not at all improbable that alcohol itself furnishes force directly, by entering into combination with the first products of tissue-decay, whereby they are again assimilated, without being excreted as urea, uric acid, etc. Many of these bodies are highly nitrogenous, and, under ·ain circumstances, might yield their nitrogen to the construction · tissues. Upon this hypothesis, and upon this alone, so far as I can perceive, can be reconciled the facts that an increase of force and a diminution of the products of the decay of tissue attend upon the ingestion of alcohol."

Another American writer, Professor A. B. Palmer, of the University of Michigan, has a communication on this subject in the *Peninsular Journal of Medicine.* I am indebted for the following abstract of Professor Palmer's paper to Dr Ross's article in the *London Medical Record*, 13th Jan. He starts with the assertion of the late Dr Anstie, that about ten drachms of absolute alcohol may be taken in a diluted form by a healthy adult in the course of twenty-four hours, without producing any noticeable disturbance of the system, or any of it being capable of detection in the excretions. Dr Anstie also thought that, if it is not oxidized in the system, of which there is no actual proof, it must undergo some unknown transformations by which a certain amount of force it set free. Professor Palmer urges as objections to these statements, that Du Roy and Perrin of Paris have averred that all the alcohol taken into the system is eliminated unchanged; and he thinks that our methods for detecting so small a quantity are so imperfect as to render it impossible to come to a reliable conclusion upon the point. And even admitting that such a transformation occurs, it does not follow that the latent force of the alcohol is converted into vital force. Also, the increased force, did it exist, would be more than counterbalanced by the narcotic action of the drug, which must have the same tendency in small as in large doses. He thinks that such theoretical arguments are not of much use either on the one side or the other; and that, in determining the question, we must have recourse to the observation of its effects upon the capacity for mental or bodily work. For proof of his position, he relies mainly upon the report of Professor Parkes, which appeared in the *Lancet* for August 1874, respecting the issue of spirit-rations in the late Ashantee war. The facts, separated from opinions, are as follow:—Of 24 total abstainers in the 42d Regiment, only 4 were ill during the whole campaign, and none of these more than once, while those who partook of the rations showed a much higher ratio of sick; the exact proportion being 1 to 6 of the former, and 4 to 6 of the latter. The difference was, no doubt, largely due to the previously more correct habits of the abstainers. In the Rifle Brigade, the ratio of admissions to the sick-list was 770 to 1000 strength, 574 being from fevers, and 196 from other causes. Of the abstainers the ratio was 400 to 1000, and all from fevers. All the witnesses testify against the utility of rum when given in the morning; but several spoke of its reviving effects when given at the end of the day. This illustrates the narcotic or anodyne power of the alcohol, but gives no evidence that it imparts even temporary strength. According to Dr Parkes, the experience of the Red River expedition "has produced a general belief that spirits were in all cases hurtful, or, at any rate, unnecessary in military service." In a hot, moist, enervating climate there is a desire for something to relieve the uncomfortable sensation produced by severe exertion; but Dr Parkes regards alcohol as the "most faithless of helps," and as a stimulant, and not as a renovator of exhausted tissues. Meat-extracts, on the other hand, act in great part by supplying to the muscles the material they want. What ground, then, have we for calling alcohol a stimulant, in the ordinary sense of that word, as that which increases vital action? And, if it does not supply materials to exhausted tissues, what grounds are there for calling it a food?

We have not space to refer at length to the admirable Cantor Lectures by Dr B. Ward Richardson; we will content ourselves, therefore, with a brief quotation, referring our readers to the *Journal of the Society of Arts*, in which these lectures are published. Dr Richardson says—"*Firstly*, I believe there is a certain determinable degree of saturation of the blood with alcohol, within which degree all the alcohol is disposed of by its decomposition. Beyond that degree the oxidation is arrested, and then there is an accumulation of alcohol, with voidance of it, in the unchanged state, in the secretions.

"*Secondly*, The change of decomposition of the alcohol in its course through the minute circulation, in which it is transformed, is not into carbonic acid and water, as though it were burned, but into a new soluble chemical substance, probably aldehyde, which returns by the veins into the great channels of the circulation.

"*Thirdly*, I think I have made out that there is an outlet for the alcohol, or for the fluid product of its decomposition, into the alimentary canal through the secretion of the liver. Thrown into the canal, it is, I believe, subjected there to further oxidation—is, in fact, oxidized by a process of fermentation attended with the active development of gaseous substances. From this surface the acid product is in turn reabsorbed in great part, and carried into the circulation, and is disposed of by combination with bases, or by further oxidation.

"Here, however, I leave the theoretical point to revert to the practical; and the practical is this, that alcohol cannot, by any ingenuity of excuse for it, be classified amongst the foods of man. It neither supplies matter for construction nor for heat. On the contrary, it injures construction, and it reduces temperature."

In *Virchow's Archiv*, Band 60, page 471, Dr Strassburgh has a paper on the effects of alcohol in fever. It appears that alcohol, like quinine, acts more powerfully in the febrile state, and its action is that of a depressor of temperature. He recommends that alcohol when administered should be given in the form of rectified spirit, diluted with water.

In a paper in the *Lancet* (19th Sept. 1874) on the comparative action of alcohol and absinthe, M. Magnan shows that the tendency of the latter is to induce epileptiform attacks.

PHYSIOLOGICAL ACTION AND THERAPEUTIC USES OF PROPY-
INE.—Dr P. Cerasi has lately published a thesis on this subject.
's practice he has found that while chlorhydrate of trimethy-

lamine is more remarkable for its sedative action on the nervous system, the chlorhydrate of amylamine more rapidly diminishes the temperature. Dr Cerasi has employed propylamine in fourteen cases of acute rheumatism with success, and he is of opinion that the drug acts beneficially in such cases, by acting as a narcotic and as a depressant on the cardiac and arterial dilatation. Cerasi has tried the chlorhydrate of trimethylamine in other affections, in which the indications of treatment are to moderate and regulate the circulation. He has thus beneficially treated four cases of imperfect cardiac innervation, two cases of hypertrophy, and one of dilatation. In a case of acute pericarditis treated by this drug, the pain was diminished, and the force of the pulsation lessened. In two cases of typhus, and one of typhoid fever, two cases of scarlatina, and one of malignant smallpox in the suppurative stage, the temperature was lowered by the use of this medicine. In two cases of pyæmia, the temperature was lowered, but in one of them the temperature suddenly rose again, and the case terminated fatally. In a case of acute alcoholism, the temperature fell under the employment of chlorhydrate of amylamine.

ACTION OF QUININE ON THE NERVOUS SYSTEM.—In the Pharmacological Institute at Bonn, Heubach has recently been investigating the above subject. The preparation of quinine used was the slightly alkaline amorphous muriate. Heubach finds from his experiments, that small doses will cause exaltation and not lowering of the reflex excitability primarily, but subsequently it causes diminution of it owing to paralysis of the heart. Not only are the respiration and activity of the heart affected by very large doses, but vital activity, and consequently reflex excitability, are rapidly destroyed. Death from quinine is not due, in the first place, to a direct poisoning of the heart, but to paralysis of the respiration. —*Centralblatt*, No. 43.

INFLUENCE OF QUININE ON SUPPURATION.—Dr Douglas Morton, physician to the Louisville Hospital, U.S., records in the *Practitioner*, Nov. 1874, cases illustrative of the control exercised by quinine over suppuration. In gonorrhœa he used an injection consisting of two and a half grains to the ounce of water, with such success that he has rejected all other methods of treatment. He also records a case of empyema, of four months' standing, successfully treated by quinine injections. Every day, after cleansing the cavity with carbolic acid lotion, six grains of quinine dissolved in two or three ounces of water were thrown in and allowed to remain. Since the commencement of this treatment, the discharge of pus rapidly diminished, and no unpleasant odour was noticeable. Another experiment was made on a large ulcer of two years' standing, situated on the leg of an ill-nourished unhealthy woman of about 40 years of age, who was suffering from mitral lesion, which had already begun to cause œdema of the feet and ankles. Quinine was applied in benzoated zinc ointment in the proportion of ten grains to the ounce. After two or three days the suppuration h greatly diminished, the whole surface of the sore was covered wit healthy granulations, and the healing process rapidly advanc Another case of mammary abscess was also successfully treated b injections consisting of ten grains of quinine in an ounce of water.

QUININE AS AN OXYTOCIC.—Dr Blackwood states (*Philadelph Medical and Surgical Reporter*, Jan.) that he has had unmistakab proofs of the power of quinine to excite the uterus to action. I cases in which quinine was administered to pregnant women wh were suffering from ague, abortion was rapidly induced. He doe not believe that intermittent fever predisposes to abortion, for i similar cases when arsenic was substituted for quinine, such accidents did not occur. Dr Blackwood has tried quinine in labour cases, and, although he found it to be uncertain in its effects, in some cases it acted as powerfully and as promptly as ergot.

Part Fifth.

MEDICAL NEWS.

A DEATH during the inhalation of ether occurred last week in the Manchester Workhouse. The patient, a youth aged sixteen, was undergoing a slight surgical operation at the hands of Dr Hardie, medical officer to the workhouse, who had previously operated twice on him, on both which occasions he had been placed under chloroform. He bore this anæsthetic so badly, that in the present case it was decided to give him ether. Soon after the administration was commenced alarming symptoms were developed, and in spite of the application of the galvanic battery, aided by artificial respiration, the patient died. At an inquest held on the body, a verdict was given, in accordance with the medical testimony, of "Died from syncope."—*Lancet.*

MR JAMES DEWAR, Demonstrator of Chemistry in the University of Edinburgh, and at one time assistant to Professors Playfair and Tait, has been elected without opposition to the vacant Jacksonian Professorship of Experimental Philosophy at Cambridge. The stipend of this office is £500 a year.

BILL TO AMEND THE MEDICAL ACT, 1858.—The following is a copy of a petition which has been forwarded to Dr Lyon Playfair, M.P., for presentation to the House of Commons, by the Royal College of Surgeons of Edinburgh :—"That a bill has been introduced into your honourable House, entituled 'A bill to amend the Medical Act, 1858, so far as relates to the registration of women who have taken the degree of Doctor of Medicine in a foreign University.' That this bill is intended to be an amendment of the dical Act, 1858, in so far as it provides that women, and women only, who have taken the degree of Doctor of Medicine in certain foreign Universities—viz., France, Berlin, Leipzic, Berne, and Zurich—shall be entitled to admission to the Medical Register established in the United Kingdom under the Medical Act, 1858. That under the Medical Act, 1858, none, either male or female, are entitled to registration upon a foreign degree, unless they had obtained such degree, and had been practising in Great Britain, before the passing of that Act. That it might be a fair question for consideration whether the degrees of certain Universities of good repute, both foreign and colonial, should be allowed the privilege of registration in this country—that privilege not being confined, as by this bill, to women; but that, were provisions to be made by Act of Parliament for such registration, it would be expedient that it should be left to the General Medical Council to decide, after due inquiry, what foreign and colonial University degrees, from the value of such degrees, should be considered equivalent to British degrees and diplomas, and should be admitted to registration under the Medical Act, 1858. That it is the more necessary that such authorized inquiry and decision by the Medical Council should be made essential, seeing that there are not only bad degrees among the foreign degrees, but in some foreign countries — Germany, for example — University degrees do not qualify for practice in these countries, unless the holders of them shall have also passed the *Staats-examen.* That without such provision, it would be unfair to the public and to the profession of Britain to admit the degrees of foreign and colonial Universities which are beyond the supervision of the Medical Council, to the same privileges in regard to registration as the licensing bodies of Great Britain, all of which are supervised through the visitation of inspectors deputed by the Medical Council, and all of which are liable to have their licensing powers suspended by the Privy Council, on representation by the Medical Council that the course of study and examination to be gone through in order to obtain their degrees or diplomas is not such as to secure the requisite knowledge and skill for the efficient practice of the medical profession. That to pass a measure such as the bill in question, brought in by private members for the avowed and sole purpose of admitting to the Medical Register women who have taken, or may hereafter take, degrees in certain foreign Universities, and that independently of inquiry and decision by the Medical Council, would be inexpedient and partial legislation, and inconsistent with the great object sought to be obtained by the existing Medical Act—viz., to enable the public to distinguish qualified from unqualified practitioners. May it therefore please your honourable House to take into consideration the reasons above narrated, and to take such means as will prevent the bill in question from being proceeded with or passed. And your petitioners will ever pray. Signed in name and by authority of the Royal College, JAMES SIMSON, President. Edinburgh, 19th April 1875."

OBITUARY.

Professor Inglis of Aberdeen.—Andrew Inglis, whose dea we have to record, was the representative in the present generati of a name long and honourably connected with medicine in Edinburgh.

As a boy he showed, with quick and lively talents, a grea aptitude for natural history and botany; and all through hi student life he worked with his whole heart at every branch o medical study, and cognate science, with a love and enthusiasm which made labour an amusement.

During the latter part of his student life, and the whole of his professional career, obstetrics, and gynæcology in general, became his ruling passion. No labour was too great for him; no case, however poor or humble, was devoid of interest in his eyes. His contributions to practical midwifery were numerous and important, so when, in 1869, he was appointed to the Chair of Midwifery in Aberdeen, his friends felt that now he had obtained a field worthy of his ambition, and entered on a career in which he would find, not only pleasure, but reward. His enthusiasm was great; his love of teaching and of practical work was apparently inexhaustible; but, alas, his health gave way, and for the last two years Dr Inglis has been unable to teach, or even to practise, and he died 13th of March, in the thirty-eighth year of his age.

In addition to the proper duties of his Chair, Professor Inglis, on being appointed one of the medical officers of the Aberdeen Dispensary, immediately set about improving the arrangements for conducting the midwifery practice connected with it, and so organizing them as to form a special department. He followed up this by an effort—successful on a small scale—to establish a lying-in hospital, no such institution having previously existed in Aberdeen. It was mainly by his personal influence and exertions that he succeeded in accomplishing an object which he considered an essential auxiliary to the proper teaching of his department in the University, and at the same time urgently required for the benefit of the poor of so large a city as Aberdeen. He, nevertheless, experienced strong opposition to his efforts, and the institution has languished ever since he was laid aside from his work, and the continuance of its existence is doubtful, unless it shall be taken up again by a new and determined strong hand.

Those of the profession who know and respect Dr Archibald Inglis—and there are none who know him and do not respect him—will deeply sympathize with him in the loss of his son, of such high promise.

TO CORRESPONDENTS.

No notice can be taken of anonymous communications. Whatever is intended for insertion must be authenticated by the name and address of the writer—not necessarily for publication, but as a guarantee of good faith.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Medical Notes about Shakespere and his Times: Being the Harveian Address to the Edinburgh Society, on April* 12, 1875. By J. D. GILLESPIE, M.D., F.R.C.S.E., Secretary to the Society.

GENTLEMEN,—In giving you a substitute for the regular Harveian oration of the year, I must premise that I only appear in the field after your President—Dr Irvine of Pitlochry, a gentleman eminently qualified to provide one—from want of leisure, declined to undertake the task, and after I had recourse to other members of the Society, whom I in vain supplicated to render us help in the time of need. I would further remark, that when I had prepared, with considerable trouble, a mass of information about the medical knowledge of Shakespere, which was the subject I had chosen for my text, I found that I had been forestalled, and that Dr Bucknill had already, in the year 1860, given to the world a work of much interest and erudite research bearing on the subject. Baulked in my choice, I have now determined on giving you a rambling, but I trust not uninteresting discourse, entitled, "Medical Notes about Shakespere and his Times." As William Harvey —the patron saint, shall I say, of our Society—was a contemporary of Shakespere, such topics should not come amiss.

I have often been struck, when reading Shakespere, with the amount of medical knowledge he displayed, and more especially with the beautiful imagery in which he clothes his references to medicine. It is not so much his portraiture of the doctor, as his poetic fancies about what some people would think could only be presented in a prosaic form, that offer such charms to the reader.

The following passages, among many others, owe their beauty to metaphorical allusion to medical subjects:—

> "The canker galls the infants of the spring,
> Too oft before their buttons be disclosed;
> And in the morn and liquid dew of youth
> Contagious blastments are most imminent."
> *Hamlet*, Act i., Scene 3.

> "Some to kill cankers in the musk-rose buds."
> *Midsummer Night's Dream*, Act ii., Scene 3.

"In the sweetest bud the eating canker dwells."
Two Gentlemen of Verona, Act i., Scene 1.

"But now will canker sorrow eat my bud."
King John, Act iii., Scene 4.

In these passages canker does not mean cancer, but a rode ulcer. The only mention of cancer by Shakespere is as one of th signs of the zodiac—

"And add more coals to Cancer, when he burns
With entertaining great Hyperion."
Troilus and Cressida, Act ii., Scene 3.

The property of some plants to contain a deadly poison, whil they also possess a charming perfume, is in the following passag thus expressed, and a corollary drawn therefrom:—

"Within the infant rind of this small flower
Poison hath residence, and med'cine power;
For this, being smelt, with that part cheers each part;
Being tasted, slays all senses with the heart.
Two such opposed foes encamp them still
In man, as well as herbs, grace and rude will;
And, where the worser is predominant,
Full soon the canker death eats up that plant."
Romeo and Juliet, Act ii., Scene 3.

The description of Juliet, lying on her bed apparently in the sleep of death, is beautiful:—

"Out, alas! she's cold;
Her blood is settled and her joints are stiff;
Life and these lips have long been separated:
Death lies on her, like an untimely frost
Upon the sweetest flower of all the field."
Romeo and Juliet, Act iv., Scene 5.

A still more striking instance of Shakespere's accurate delineation of approaching death is to be found in Mrs Quickly's account of the death of Falstaff:—

"Nay, sure, he's not in hell; he's in Arthur's bosom, if ever man went to Arthur's bosom. 'A made a finer end, and went away, an it had been any chrisom child: 'a parted even just between twelve and one, e'en at turning o' the tide: for after I saw him fumble with the sheets, and play with flowers, and smile upon his fingers' ends, I knew there was but one way; for his nose was as sharp as a pen, and 'a babbled of green fields."—*Henry V.*, Act ii., Scene 3.

This so closely resembles a passage in Hippocrates, that commentators have held it proved Shakespere had studied his writings, or got his information second-hand; but, with all due deference to these great authorities, I would suggest that the poet had himself witnessed a deathbed, and his faithful memory furnished the necessary food for his graphic pen.

It is questionable whether Shakespere was acquainted with the discovery of Harvey about the circulation. There is certainly no passage in his works that can unmistakably prove such knowledge; but he was evidently well up in the current belief of the day. In the beginning of "Coriolanus," Æsop's fable of the Belly and the other Members, gives us a fair idea of what was believed about the distribution of the blood; and, in Act v., we have a capital illustration of what was considered the effects of a good dinner on the blood primarily, and on the temper secondarily. Menenius says—

> "He was not taken well; he had not dined:
> The veins unfilled, our blood is cold, and then
> We pout upon the morning, are unapt
> To give or to forgive; but, when we have stuffed
> These pipes and these conveyances of our blood
> With wine and feeding, we have suppler souls
> Than in our priest-like fasts: therefore I'll watch him
> Till he be dieted to my request,
> And then I'll set upon him."

If we study the rôles of the different doctors and apothecaries drawn by Shakespere, we do not find much to throw light upon the amount of medical knowledge he possessed. It is more from observations casually dropt by other characters that we gain our information. It may not be out of place to sketch shortly the parts played by these different medical men. In six of the plays out of a total of thirty-six, medical characters are found.

1. In the "Merry Wives of Windsor," Dr Caius, a French doctor, comes upon the scene. He is evidently introduced merely as a sort of foil to the Welsh parson, Sir Hugh Evans, and there is no proof that he was the counterpart of any living physician at the time; though he has been fathered upon two celebrated men, viz., Dr Caius, who founded Caius College, Cambridge, and Sir Theodore Mayerne, a Frenchman, who was expelled by the French College of Physicians from their body, because he advocated the new chemical theory of disease, in opposition to their favourites Galen and Hippocrates. He afterwards came to London, settled there, and acquired great eminence. It is possible that, in Shakespere's time, there were Frenchmen, having only a very imperfect knowledge of English, practising medicine or quackery in London, whom he wished to pourtray. That Caius was a quack we may infer from Sir Hugh Evans calling him "Master Caius, that calls himself Doctor of Physick." Moreover, he says, "I will knog his urinals about his knave's costard." It was the most common quackery of the day to pretend to diagnose disease from casting, as it was called, the urine.

How much value was set upon examination of the urine, and how frequently it was practised, may be gathered from a perusal of many publications of the period. In the story of "Dorastus and Faunia," on which the play of "Winter's Tale" was founded, the following passage occurs:—

"Wherefore he caused physitions to come, but Dorastus would neither let them minister, nor so much as suffer them to see his urine."

In the novel of "Apollonius, Prince of Tyre," which was the original of "Pericles," we read—

"When the king perceived his daughter's infirmitie to increa he sent immediatlie throughout all the dominions for the learne phisitions to search out her grief and to cure it, who examining h urine, and feeling her pulse, could find out no manifest cause substance of her disease."

In Stow's "Chronicle," it is recorded that a water-caster w punished for exercising his quackery. He was set on horseback with his face to the tail, which he held in his hand, with a coll of urinals about his neck, led by the hangman through the cit whipped, branded, and then banished.

Tenant, a urine-caster, sold pills at £6 each. We are told b John Halle, when speaking about a quack of the name of Valentine, but calling himself Master Wynckfylde,—"When anye came to him wyth urines (whyche commonlye in the country they brin in a stone cruse), he made them believe, that onelye by feling the weight thereof, he would tell them all theyre diseases in their bodies, or wythout, and otherwhile make them believe that h wente to ask councel of the devel," etc.

In the Second Part of "Henry IV.," we have Falstaff asking his page—

> "FALSTAFF.—Sirrah, you giant! what says the doctor to my water?"

The page's reply is most felicitous—

> "PAGE.—He said, sir, the water itself was a good healthy water; but for the party that owed it, he might have more diseases than he knew for."

Again, in "Twelfth Night," Fabian says, speaking of Malvolio—

> "Carry his water to the wise woman."

Women at that period were often irregular practitioners.

We may dismiss Dr Caius with the remark, that he must have been plausible and of good address, or he would not have succeeded in inducing so clever a woman as Mrs Page to connive at his attempted abduction of her daughter.

2. In "Macbeth," we have both an English and a Scotch doctor, the former saying four lines and a half in the whole play, which might just as well have been spoken by any one else.

> "DOCTOR. Ay, sir: there are a crew of wretched souls,
> That stay his cure: their malady convinces
> The great assaye of art; but, at his touch,
> Such sanctity hath heaven given his hand,
> They presently amend."

This alludes to a supposed virtue possessed by the King of England of curing the king's evil, or scrofula; and as doctors were said to be helpless in that trouble, there was little necessity for the introduction of one to speak to the king's supernatural powers.

The Scotch doctor makes a better show on the stage; for he exhibits astuteness, caution, and a regard for number one—three qualities often ascribed to us by our southern brethren.

His astuteness comes out in the Somnambulistic scene, where he gathers from Lady Macbeth's guilty utterances the nature of her crimes—

> "DOCTOR. More needs she the divine than the physician."

His caution appears in the line—

> "DOCTOR. I think but dare not speak."

And his Scotch canniness may be gathered from this—

> "DOCTOR. Were I from Dunsinane away and clear,
> Profit again should hardly draw me here."

3. In "Henry VIII.," Dr Butts, the king's physician, plays a noble part, for he it is that goes to tell the king of the way in which Cranmer was treated. The friendly and intimate relations between the king and his physician come forcibly out in the passage.

Though called Dr Butts, it is well known that Dr Butte was Court Physician to Henry VIII.

4. Cornelius, the physician in "Cymbeline," had a very difficult part to play. He was asked by the Queen, who had imbibed a passion for wholesale poisoning, to provide her with a deadly drug. He suspected her purpose, but when he asked her what she wanted to do with it, he was foiled by her plausible reply. She told him he had already taught her the properties of simple remedies, and it was natural she should like to advance a step further in science, and experiment with poisons. Of course, she would only practise on the inferior animals. It was just the modern story, getting arsenic to kill rats. It is hard to say what course Cornelius could have pursued other than he did. He durst not on mere suspicion accuse the Queen. The loss of his own head would probably have been the penalty. I presume, if her Britannic Majesty were to ask one of her Physicians to the Queen for Scotland to procure her a phial of morphia, he would accede to her request. Cornelius did the best he could under the circumstances; he gave her a comparatively innocent drug, which produced for a time the semblance of death, but from which the patient revived to live with increased vigour. It is true that no such drug was known in Shakespere's time; but it is quite legitimate for Shakespere, in a work of fiction, to suggest a plant possessing such properties. In "Romeo and Juliet" the same expedient is resorted to, but a friar and not a physician possessed the secret.

5. The physician in "King Lear" is made to play a very important part, and does it well. The old king, driven to madness by the behaviour of his two elder daughters, is put under his care by Cordelia. The means of cure, though simple, are natural enough. He gets a strong soporific, and when he wakens from it, while soothed by the strains of music, his favourite daughter is told to speak to him, and draw him from his disordered fancies. The

physician shows his tact in the following lines, for, satisfied with the good already produced, he does not allow the interview to be prolonged :—

"**Physician.** Be comforted, good madam ; the great rage,
You see, is cured in him, and it is danger
To make him even o'er the time he has lost ;
Desire him to go in, trouble him no more,
Till further settling."

In several other passages Shakespere alludes to the power of music as a soother of a distressed or troubled mind :—

"**Lucentio.** Preposterous ass ! that never read so far
To know the cause why music was ordain'd !
Was it not to refresh the mind of man
After his studies, or his usual pain ?"
Taming of the Shrew, Act iii., Scene 1.

"**Peter.** When griping grief the heart doth wound,
And doleful dumps the mind oppress,
Then music with her silver sound,
With speedy help doth lend redress."
Romeo and Juliet, Act iv., Scene 5.

"**Duke.** If musick be the food of love, play on;
Give me excess of it, that, surfeiting,
The appetite may sicken, and so die.—
That strain again ;—it had a dying fall ;
O, it came o'er my ear like the sweet south,
That breathes upon a bank of violets,
Stealing and giving odour.—Enough ; no more ;
'Tis not so sweet now as it was before."
Twelfth Night, Act i., Scene 1.

A very different effect, however, is alluded to by Shylock, the Jew—

"And others, when a bagpipes sings i' the nose,
Cannot contain their urine."
Merchant of Venice, Act iv., Scene 1.

6. The starving apothecary in "Romeo and Juliet," who sold the poison to Romeo for forty ducats, is not a faithful illustration of that class of men in Shakespere's times, for they were well-to-do and got good fees ; but the Italian novel, from which the poet obtained his plot, described the character, as it might have been observed at Verona, which was the scene of the tragedy.

"**Apothecary.** My poverty, and not my will, consents.
Romeo. I pay thy poverty, and not thy will."
Romeo and Juliet, Act v., Scene 3.

These are all the medical characters introduced by Shakespere to his plays; but we ought, perhaps, to include among the number, Helena, daughter of Gerard de Narbon, a famous French physician, in "All's Well that Ends Well," for she cures the king of a fistula, which the congregated college of physicians had pronounced incurable. Secret remedies were in those days in vogue, even among physicians of repute; and the prescription which proved so valuable, was one left her by her father.

It must be admitted that the king showed a praiseworthy reluctance to try a secret remedy, but the importunity of the lady won the day; and no wonder, for few could resist the beautiful language she employed:—

> "KING. Art thou so confident? Within what space
> Hop'st thou my cure?
> HELENA. The greatest grace lending grace,
> Ere twice the horses of the sun shall bring
> Their fiery torches his diurnal ring;
> Ere twice in murk and occidental damp
> Moist Hesperus hath quench'd his sleepy lamp;
> Or four-and-twenty times the pilot's glass
> Hath told the thievish minutes how they pass;
> What is infirm in your sound parts shall fly,
> Health shall live free, and sickness freely die."

Were the modern surgeon to use such persuasive and poetic imagery to induce his patient to submit to the operation for fistula in ano, the supposed pain would become a pleasure. Let us hope that when, some years hence, an erudite and accomplished class of female physicians and surgeons has been established, their cures may be as perfect, as simple, and as gracefully achieved, as by Helen of Narbon.

Investigation with respect to Shakespere and his plays has now disclosed an amount of information peculiarly interesting to his admirers. The work entitled, "Shakespere's Library," a publication by Mr Collier of the various stories and plays from which our great dramatist culled several of his plots, is replete with entertaining matter. I don't suppose many persons are aware that the "Merry Wives of Windsor" owes the general character of the plot to a novel entitled "The Two Lovers of Pisa," printed in the "News out of Purgatorie," a work by Tarlton, who was a celebrated actor in Shakespere's time.

Shakespere, with infinite tact, has reversed the characters of the jealous husband and the lady's lover, and extracted a wholesome moral, for no one pities Falstaff when he suffers the indignities we read of in the play; while in the novel, the laugh is chiefly at the expense of the aged and jealous husband. By his description you will see that he belonged to our profession, and was selected to marry a beauty on account of his wealth. "At last an olde doctor in the towne, that professed phisicke, became a sutor to her, who was a welcome man to her father, in that he was one of the welthiest men in all Pisa. A tall stripling he was, and a proper youth, his age about four score; his head as white as milke, wherein for offence sake there was left never a tooth; but it is no matter, what he wanted in person he had in the purse, which the poore gentlewoman little regarded, wishing rather to tie herself to one that might fit her content, though they lived meanly, than to him with all the wealth in Italye."

A young cavalier falls in love with the wife, not knowing who her husband was, and innocently consults the venerable doctor. This leads to incidents closely resembling the escapes of Falstaff, the difference being, however, that the lady favours the advances of her lover. In the end the doctor dies, and the lady and her lover are united. "The jest went so to his hart that he shortly after died, and Lionello enjoyed the ladye, and, for that they two were the death of the old man, now they are plagued in purgatory, and he whips them with nettles."

These five words, "He whips them with nettles," probably gave rise to the amusing scene of the fairies at Herne the Hunter's oak, where Falstaff is burnt with tapers, and baited by the whole company.

It must be a gratification to our profession to know that Shakespere's play of "As You Like It" was founded on a novel written by a medical man. Dr Thomas Lodge began life as an actor, a poet, and a dramatic author, and the novel of "Rosalynd" was written by him "to beguile the time" while on a voyage to the Canaries. It was published in 1590; and about six years afterwards, he seems to have taken up the science of medicine, having previously been a student at Lincoln's Inn. In 1603, he published a treatise on the plague, to which I shall hereafter allude. The plot in Lodge's "Rosalynd" is adhered to pretty closely by Shakespere, and some of the poetry, so liberally interspersed throughout the story, will compare favourably with similar passages in "As You Like It."

The first sonnet in Lodge's work, on the praises of Rosalynd, was engraved by Rosader on the bark of a "*mir tre,*" and runs thus:—

SONNETTO.

"Of all chast birdes the phœnix doth excell,
Of all strong beastes the lion beares the bell,
Of all sweet floures the rose doth sweetest smel,
Of all fair maydes my Rosalynd is fairest.

Of all pure mettals gold is onely purest,
Of all high trees the pine hath highest crest,
Of all soft sweets I like my mistris brest,
Of all chaste thoughts my mistris thoughts are rarest.

Of all proud birds the eagle pleaseth Jove,
Of pretie fowles kind Venus likes the dove,
Of trees Minerva doth the olive love,
Of all sweet nimphs I honour Rosalynd.

Of all her gifts her wisedom pleaseth most,
Of all her graces virtue she doth boast:
For all these gifts my life and joy is lost,
If Rosalynd prove cruell and unkind."

The corresponding passage in "As You Like It" is as follows:—

"ROSALIND. From the east to western Ind,
No jewel is like Rosalind.
Her worth, being mounted on the wind,
Through all the world bears Rosalind.
All the pictures, fairest limn'd,
Are but black to Rosalind.
Let no face be kept in mind,
But the fair of Rosalind."[1]

You may recollect what a poor opinion of these lines Touchstone the clown had, and what a clever imitation he produced; he also said—

"CLOWN. I'll rhime you so, eight years together,
Dinners and suppers and sleeping hours excepted:
It is the right butterwoman's rate to market."

The wooing scene between Rosalind and Orlando in "As You Like It," has its prototype in "The wooing eglogue betwixt Rosalynde and Rosader," which is very neatly rendered. I shall quote one passage that appears to me creditable to the author:—

"ROSADER. I pray thee, Rosalynd, by those sweet eyes,
That stain the sun in shine, the morne in cleare,
By those sweet cheeks where love incamped lyes
To kisse the roses of the springing yeare.
I tempt thee, Rosalynd, by ruthful plaints,
Not seasoned with deceipt or fraudfull guile,
But firm in payne, far more than toong depaints,
Sweet Nymph; be kind, and grace me with a smile.
So may the heavens preserve from hurtful food
Thy harmless flocks; so may the summer yeeld
The pride of all her riches and her good,
To fat thy sheepe (the cittizens of field).
Oh! leave to arme thy lovely browes with scorne:
The birds their beake, the lyon hath his taile,[2]
And lovers nought but sighs and bitter mourne,
The spotlesse fort of fancie to assaile.
Oh! Rosalynde, then be thou pittifull,
For Rosalynde is onely beautifull," etc.

The shepherd Sylvius, whom Phœbe scorns, in "As You Like It," owing to her misplaced affection for Ganimed, is represented in Lodge's play as Montanus, and I must quote the first stanza of his sonnet to her. The novel narrates: "At gaze upon this gorgeous nymph sate the shepheard, feeding his eyes with her favours, wooing with such piteous lookes, and courting with such deepe strained sighs as would have made Diana herselfe to have been compassionate: at last, fixing his lookes on the riches of her face, his head on his hande, and his elbow on his knee, hee sung this mournefull dittie:—

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[1] Fair means beauty, or complexion.
[2] Taile here, I think, means talon.

MONTANUS SONNET.

"A turtle sat upon a leavelesse tree,
Mourning her absent pheare,[1]
With sad and sorry cheare:
About her wondring stood
The citizens of wood,
And whilest her plumes she rents,
And for her love laments,
The stately trees complaine them,
The birds with sorrow paine them.
Each one that doth her view,
Her paine and sorrowes rue;
But were the sorrowes knowne,
That me hath overthrowne,
Oh, how would Phœbe sigh, if shee did looke on me," etc.

Another song of Lodge's is evidently the suggesting idea to Shakespere in the carol which he makes the two pages sing to Touchstone:—

SONG.

"It was a lover, and his lass,
With a hey, and a ho, and a hey nonino,
That o'er the green corn field did pass
In the spring time, the pretty rank time
When birds do sing, hey ding a ding, ding;
Sweet lovers love the spring.

Between the acres of the rye
With a hey, and a ho, and a hey nonino,
These pretty country folks would lie,
In the spring, etc.

The carol they began that hour,
With a hey, and a ho, and a hey nonino,
How that life was but a flower
In the spring time, etc.

And therefore take the present time,
With a hey, and a ho, and a hey nonino;
For love is crowned with the prime
In the spring time," etc.

Lodge's version is:—

CORIDON'S SONG.

"A blythe and bonny country lasse,
Heigho, the bonny lasse!
Sate sighing on the tender grasse,
And weeping said, Will none come woo me?
A smicker boy, a lither swaine,
Heigh ho! a smicker swaine!
That in his love was wanton fain,
With smiling looks straight came unto her.

---

[1] Pheare or fere, a companion.

When, as the wanton wench espied,
Heigh ho, when she espied!
The means to make herselfe a bride,
She simpred smooth like bonny bell:
The swaine, that saw her squint eied kind,
Heigh ho, squint eyed kind!
His arms about her body twined,
And, faire lasse, how fare ye, well?

The country kit said, Well forsooth,
Heigh ho, well forsooth!
But that I have a longing tooth,
A longing tooth that makes me crie:
Alas! said he, what garres thy grief?
Heigh ho, what garres thy grief?
A wound, quoth she, without reliefe,—
I fear a maid that I shall die.

If that be all, the shepheard said,
Heigh ho, the shepheard said!
He make thee wive it gentle mayd,
And so recure thy maladie,
Hereon they kist with many an oath,
Heigh ho, with many an oath!
And fore God Pan did plight their troath,
And to the church they hied them fast.

And God send every pretie peate,[1]
Heigh ho, the pretie peate!
That feares to die of this conceate,
So kind a friend to helpe at last."

I must not dwell longer on the novel of "Rosalynde," except to say that, though the novel falls very far short of the comedy, it displays much inventive genius, and was adhered to pretty closely as the model on which the bard of Avon constructed one of his most celebrated plays. Let us turn now to what Dr Lodge has to say about the plague, which was very deadly in London when he published his treatise, in 1603.

Various opinions were held as to its origin, and Lodge sets himself to combat especially the very popular one, that it was caused by the heavenly bodies. His proposed remedies are numerous, being indeed a compilation of what had been suggested by many other physicians, and consisted chiefly of a combination of a great many vegetable substances. There was, however, one jewel, the Eastern hyacinth, which, on the authority of Avicen, he specially recommended to be worn on the breast, or, still better, to be kept in the mouth, as it had the property of "resisting all venoms." The perfume he proposes "to aire the chamber of him that is infected, correcting the venomous aire," though more complicated, gives us a foretaste of carbolic acid spray:—

"Take black pitch, rosin, white frankincence, of each five ounces; of mirrhe four ounces; of the wood of aloes half a dramme; of storac and benjamin, of each a dramme; of juniper berries, and the leaves of rosemary, of each two drammes; make a grosse powder of these, and in a chafing-dish and coales cast off the same, and perfume the chamber." He even seems to have had a foreshadowing of the tubular tents now in fashion; for, when speaking of the treatment of the carbuncle after it has been opened, he says, "For mine own part I am truly assured that it is far better to use certaine tents of hollow silver, lead, or tinne, then of lint altogether, to the end that by the hollow tents, the venom may the better and the sooner be evacuated, and not stayed within, which is the intention that a good and advised surgeon ought to have. And this may serve for advise and counsaile henceforward."—Here is surely Professor Lister himself, only lacking the elegance!

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[1] Peate, a delicate person, usually applied to a young female.

"Take of the mundification of rozen, and put it uppon the saide sores within them by hollow tents."

It is difficult to conceive how a man, who showed such common sense in most of his remarks about the plague, should have adhered to some of the absurd superstitions of the age; for after having mentioned several reasonable remedies for inducing the maturation of the plague carbuncle, he says, "It is likewise very allowable to draw out the venime from the sore to take a chicken or cocke, and to pull the feathers from his taile, and to apply him to the soare, for, by this meanes, he draws out the venome, and when he is dead apply another. Instead of this remedy, some use to take great pullets and pigeons, and cutting them in two along the backe, apply them hote as they are upon the tumour or carbuncle." It is still more surprising that in 1665, sixty-two years afterwards, "the Colledge of Physicians," giving "certain necessary directions as well for the cure of the plague as for preventing the infection," should say, "These following are very good:—Pull off the feathers from tails of living cocks, hens, pigeons, or chickens, and holding their bills, hold them hard to the botch or swelling, and so keepe them at that part until they die, and by this means draw out the poison." Well might Lord Bacon, a contemporary of Lodge's, say, when speaking of the advancement of medicine, "I find much iteration but small addition in writers of that faculty."

Sydenham, now justly regarded as the father of modern medicine, though he improved greatly on the practice not only of the ancients, but of his contemporaries, seems in some degree, if I may use the expression, to have been tarred with the same stick. Look at his treatment of ileus:—"I order, too, that meanwhile a live kitten be kept continually lying on the naked belly."—"The kitten is not to be taken off before the patient begins with the pills."

In the treatment of continued fever, in paragraph 40, you will find a still more objectionable practice recommended:—

"Having had recourse to other remedies without success, I was compelled to turn my thoughts towards new quarters, and to apply to the patients the lively and vigorous warmth of young people."

have never found that the repeated application of warm flannels has ever had the effect of the plan I have described." "Now this principle of transfusing into the body of the patient effluvia and exhalations, which may perhaps be of a balsamic nature, although at first it may seem strange, has been applied from the time that I first adopted it by others as well as myself, and that with good results; and I am by no means ashamed of it, even although some few from amongst the arrogant, impertinent, and supercilious, despisers of everything common, may sneer at me by reason of it." It is hardly fair in Sydenham to claim for himself the merit of this wonderful remedy in continued fever, when he must have stolen the idea from the selection of Abishag, the Shunammite woman, to lie in David's bosom to keep him warm.

Leaving Sydenham, let us return to the plague pamphlets. In 1665, sixty-two years after Lodge's pamphlet, John Gadbury, who styled himself Philomathematikos, and at the very same time that the College of Physicians published their instructions, brought out a pamphlet entitled "London's Deliverance Predicted, in a Short Discourse, showing the Causes of Plagues in General," etc. This writer made out, to his own satisfaction, that celestial causes gave rise to the plague, more especially the unfortunate conjunction of Saturn, Jupiter, and Mars.

In the same year another treatise appeared, "The Shutting up of Infected Houses, as it is practised in England, Soberly Debated." The writer was anonymous, and gave eight reasons for not separating the sick—none of them of much value. He also has a list of seventy-five ways in which men got the plague in 1665, such as, 1st, A whole family becoming infected by one's overheating himself and drinking small-beer upon it; 2d, By eating a pound of red cherries for a wager; 3d, By a coat one bought of a broker behind St Clement's Church; 4th, By a sink shut up by reason of the water not running in their back side (a very likely cause!); 8th, By an infected hackney coach; 9th, By an infected sedan; 18th, By a cat in catterwouling; 69th, By money taken for physick administered, etc. One would have thought that these assigned reasons would have convinced the writer of the propriety of establishing a quarantine.

I am sorry to be obliged to avoid almost entirely mention of Lord Bacon, also a contemporary of Shakespere. In his book on the "Advancement of Learning," he devotes a chapter to medicine, well worthy the perusal of modern physicians; but to do it justice, would require an essay devoted entirely to the work. He shows in a forcible manner the errors of the physicians and surgeons of his time, and points out distinctly the rational view of medicine, so happily adopted subsequently by Sydenham. What can be more true than the following observation:—"Almost all other arts and sciences are judged by their power and operation, and not by their success and work. The lawyer is judged by the virtue of his pleading, and not by the issue of the cause; the master in the ship approves his art by the directing his course aright, and not by the fortune of the voyage; but the physician, and perhaps the politick, hardly have any proper particular acts whereby they may make a clear demonstration of their art and abilities, but bear away honour or disgrace principally from the event, which is ever an unequal judicature. For who can tell, if a patient die or recover, or if a state be preserved or ruined, whether it be by art or accident? Therefore it often falls out that the impostor bears away the prize, virtue the censure. Nay, the weakness and credulity of man is such, as they often prefer a mountebank or witch before a learned physician. Therefore the poets were clear and quick sighted, when they made Æsculapius and Circe brother and sister, both children of the Sun."

The remark of Bacon previously quoted is again illustrated in the works of Peter Lowe, "Scottishman," who published in 1634 his "Discourse of the Whole Art of Chyrurgerie." Before entering on special branches of surgery, he gives the doctrines of Hippocrates and Galen unadulterated, though Hippocrates lived about 2000 years and Galen 1500 years previously. I shall just cite one instance; he mentions the four humours, the blood, the flegm, the choller, and melancholy; the blood reigning from 3 to 9 A.M., the choller from 9 A.M. to 3 P.M., the flegme from 3 P.M. to 9 P.M., and the melancholicke from 9 P.M. to 3 A.M.

A rare tract, edited by Dr T. J. Pettigrew, and printed by the Percy Society, gives us a good picture of the state of medicine in the year 1565. It is from the pen of John Halle, a surgeon at Maidstone, and is entitled "An Historical Expostulation against the Beastlye Abusers, both of Chyrurgerie and Physyke in our Tyme." Whether he was so or not, has, so far as I know, never been made out, but the coincidence of the name, and the date of his birth, 1530, render it not unlikely that he was the father of the John Halle who married Shakespere's daughter in 1607, he being then forty years of age. If this conjecture be correct, John Halle, senior, would have been thirty-seven years of age when John Halle, junior, was born. He seems to have had a very strong antipathy to quacks, and he gives a very circumstantial account of some of them, their forms of imposture, and what subsequently befell them. He first speaks of a child attended by "a beastly forme of a woman," who had a suppurating tumour in his navell, from which at last issued seven worms and a yellow substance. "The doctor and the woman fell foul of [illegible] and he was di[illegible]fited; but in the sequel "the sayde [illegible]ling to m[illegible]ie, after three days ran away, sh[illegible]ng mat[illegible] their hoste where they lay, of t[illegible]res, an[illegible]hat they lay in; and by their e[illegible] of th[illegible]vauntes of their [illegible]lle ser[illegible]nsteade of [illegible]part." [illegible]ant h[illegible] sted at that time, 1555, when Dr Bucknill tells us, that in 1551 Valsingham, Queen Elizabeth's great statesman, wrote to the ollege of Physicians in behalf of one Margaret Kennix, by Her ajesty's command, "signifying how, that it was Her Highness's leasure, that the poore woman shoold be permitted by you quietly o practise and minister to the curing of diseases and wounds by he means of certain simples, in the applieing whereof it seems hat God hath given her an especiall knowledge." In this herb-ractice, it appears she "was restrained by the College, contrary to Her Majesty's pleasure." Though threatened by the minister, the ollege held out, and appears to have been too powerful for the ecretary.

The next disturber of John Halle at Maidstone was a Robert Haris, "professing and pretendyng a hyghe knowledge in physike; nder cloke whereof he deceaved mervayloualie with vyle sorcerie." This time it was the patient he encountered. "When I reasoned vith hir agaynst his doynges, she ernestlie affirmed that she knew vell that he was then dystant from hir, at the leaste vii. myles, nd yet she verelye beleved that he knew what she then sayde. Oh! greate beastynes and infidelitie, specially in suche as have orne a face to favour the worde of God."

The third impostor was a Thomas Lufkyn, by occupation a uller and burler of cloth. He set up a bill—

> "If anye manne, womanne, or childe bee sicke, or would be let bloud, or be liseased with anye maner of inward or outwarde grefes, as al maner of agues, r fevers, plurisis, cholyke, stone, strangulion, impostumes, fistulas, kanker, goutes, pocks, boneache, and payne of the joynts, which commeth for lacke f bloudlettyng, let them resorte to the signe of the Sarazens Hedde, in the aste lane, and brynge theire waters with them to be seene, and they shall ave remedie By me, THOMAS LUFFKIN.

"Unto this devill incarnate resorted all sortes of vayne and un-liscrete persons, as it were to a God." The end of this man was, 'he sodainely was gone," etc., "wyth manye a poore mannes nonye."

The fourth empyric managed to get hold of three wives, was liscovered, imprisoned, and whipped. Nevertheless, we hear of nim at a parish called Loose: "A blynde man, named blind Orgar, nad a wyff who was sycke of dyvers aches and swellyngs, who, hearyng of this marveielous monster, sente his daughter upon a Wednesday downe to Marden with hir water, to this Maister Wynkfeld, who so inchaunted hir, that she forgate hir waye home to hyr father and mother, in so much, that her mother thoughte her loste, for she taried there tyll the Saturdaye folowyng." After talking of some others, he finishes his list of quacks "with Grigge, the poulter, and a joyner in London, a Frencheman borne, of late become a physitien;" the latter more likely to be the type of Shakespere's Dr Caius than Sir Theodore Mayerne. Next follows a quaint instruction in verse to all young chirurgeons. The advice about consultations is good and curious:—

4.

"When thou art callde at anye time,
A patient to see;
And dost perceive the cure to greate,
And ponderous for thee:

5.

See that thou laye disdeyne aside,
And pride of thyne owne skylle:
And think no shame counsell to take,
But rather with good wyll

6.

Gette one or two of experte men,
To helpe thee in that nede;
And make them partakers with thee
In that worke to procede.

7.

For in so doinge, thine honestye
Thou shalt well kepe and save;
Also thy patiente therby
Righte greate comforte shall have.

. . . . .

9.

And also if oughte goe a wrye,
Or hinder in thy cure,
The one maye mende the other's faulte,
While frendship dothe endure.

10.

The wounded or sore man also,
Shall have no cause to grudge
In you such uniformitye,
Whyle he may see and judge.

. . . . .

25.

But one thinge note, when two or moe
Together joygned be;
About the paynfull patient,
See that ye doe agree.

26.

See that no discorde doe arise,
Nor be at no debate;
For that shall sore discomfort hym,
That is in sycke estate.

. . . . .

29.

Wherefore what so ye have to saye,
In things aboute your arte;
Let it be done among your selves,
In secrete and a parte.

30.

Wyth one consent uniformlye
Comforte the wounded man;
But unto some good frend of hys
Expresse all that ye can."

. . . . .

It is gratifying to find such excellent consultation rules laid down at so early a period.

Dr Halle, who married Shakespere's daughter Susannah, and who most probably attended his father-in-law in his last illness, wrote out in Latin, "Select Observations on English Bodies of Eminent Persons in Desperate Diseases." He seems to have had what we would call a first-class practice. Dr James Cook, author of the "Marrow of Chirurgery," translated this work, adding a hundred counsels of eminent physicians collected by himself. Mr Small, the esteemed Librarian of the Edinburgh University, has already noticed this book in the *Edinburgh Medical Journal*, so my remarks shall be brief.

OBSERVATION XII. is as follows:—"John Emes, of Alcester, aged 15, was cured of incontinence of urine thus: ℞ The windpipe of a cock, dried and made into powder, and with crocus martis given in a new egg every morning." As incontinence of urine in children is often most difficult to treat, I recommend this simple remedy to the notice of my medical brethren.

OBSERVATION XXXIII.—"Brown, a Romish priest, labouring of ungarick fever, in danger of death, was cured as follows." Here follows a host of remedies, the first producing five vomits and four stools. Latterly his meat was besprinkled with a powder of "prepared pearl, prepared coral, burnt hartshorn, prepared granats, fragments of jacynt, smardines, and rubies, and a leaf of leaf-gold." The termination of the case is neatly told. "By these, beyond all expectation, the Catholick was cured, especially with the decoction of hartshorn, with which I have cured these and other fevers in a short time, very many." This poor man got forty-eight different ingredients sent through him, and it is most likely he would have got well sooner had forty-seven of them, at all events, been omitted!

OBSERVATION XLVII.—"My Lady Rainsford (beautiful, and of a gallant structure of body), near 27, was, three days after her being laid of a child, miserably tormented with pain in her belly, from which I delivered her with the following:—℞ The white of hen's dung, ʒj., being put in beer and sugar, she took it. A hot medicated poultice was also applied. By these she was delivered." Dr Cook naively remarks, "The translator hath freed several from this distemper with chamomel posset-drink."

OBSERVATION XLIII.—"One of Northampton, aged 65, had heat of urine, strangury, and an ulcer at the neck of the bladder." For this he was treated with a turpentine potion, and a complicated injection, besides a lot of tablets, composed of many ingredients. A leaden pipe was for many days passed into the urethra, smeared with unguent rubrum, and retained as long as he could. You will not be surprised to learn that under this treatment his testicles swelled, and were anointed with unguent de minio, while he drank decoction of sarsaparilla.

The sequel is instructive: "Thus was he freed from the heat of his urine; but now, being vexed with a virulent gonorrhœa, he got for ten days a powder of sarsaparilla, guaiacum, senna, dodder, hellebore, and cinnamon, and became well." He then rode to London, and had a relapse, and was treated by Dr Harvey, who cured him by an injection and an electuary. "He then went to St Vincent's well, drank the waters, but thereafter was hurt by a chirurgeon forcing in a pipe to cure a caruncle, relapsed again, and never was cured."

I come now to the diary of the Rev. John Ward,[1] Vicar of Stratford-upon-Avon. The diary extends from 1648 to 1679, and contains, among a lot of heterogeneous materials, some valuable allusions to Shakespere and his time. It is by him we are told that Shakespere died of a fever brought on by a merry meeting with Drayton and Ben Jonson. In 1661 he was in London attending lectures on anatomy at Barber Chirurgeon's Hall. He says: "Dr Scarborough had a wooden man, wherein the muscles, with all their motions, were represented; itt cost him ten pounds. He brought it to an anatomie one day, and it was stolen from him; he declared in open hall, that if any man could help him to itt again he would give him five pounds." From him we gather that there were only two anatomies in a year at the Barber Chirurgeon's Hall, besides private ones.

A very few quotations from the diary are all that I can transcribe.

"Dr Conyers dissected a person not long since that died for love in London, and they found (at least as they fancied) the impression of a face made upon his heart."

"There is a report of Hyppocrates, as if hee should say this in charge with physitians, that they should cure others with simples and compounds, and themselves with sack and claret."

"I have heard a story of Dr Syngleton, in Oxford, that when several persons had wrote to him about a place, some for one person and some for another, hee took a pair of gold scales and weighed the letters, which weighed heaviest; so hee had the place whose letters were heaviest."

It is somewhat curious that those who issue modern testimonials seem to think, judging from their voluminous character, that Dr Singleton's plan is still in vogue.

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[1] Diary of the Rev. John Ward, A.M., Vicar of Stratford-upon-Avon. From the original MSS., preserved in the Library of the Medical Society of London. By Charles Severn, M.D. London, 1839.

"One askt Dr Orboston how he lived so long in health; hee made this answer, 'Parce cœnavi, solus cubavi, curas fugavi.'"

"Dr Chamberlayne, the man-midwife, lives in the Abbey Churchyard. His fee is five pounds, yett I heard, if he come to poor people, hee will take lesse."

"A physitian told his patient this story: 'Friend, thou hast two diseases, and whilst I kill one, the other will kill thee.'"

"I have heard that Mr Shakespeare was a natural wit, without any art at all; hee frequented the plays all his younger time, but in his elder days lived at Stratford, and supplied the stage with two plays every year, and for itt had an allowance so large, that he spent at the rate of £1000 a year, as I have heard."

"I have heard of a wench that had the falling sickness many years; sometimes three, four, or five times in a day; yet when she was askt in the church, her fitts immediately left her. I heard of this at Higham, in Northamptonshire."

"Dr Sydenham is writing a book, that will bring physitians about his ears, to decrie the usefulness of natural philosophie, and to maintaine the necessitie of knowledge in anatomie in subordination to physick."

"I have heard of a physician that used constantly to fast, or else bee drunk, once every month, for the preservation of his health."

"Gill said his Mr Day hath amputated five arms, three legges, and somewhat else, since he came to Oxford, and but two of all these died, and one was a person of sixty years att least."

Barnaby Rich was a character who lived in Shakespere's time, about whom I may say a few words. He was a sort of antithesis of Shakespere's Thersites. Thersites railed at everything good; Rich railed at everything evil. He was a man of very considerable ability, had been for forty years a soldier in the time of Queen Elizabeth and King James, and had used his eyes and his ears to good purpose. He wrote a number of pamphlets discussing the current subjects of the period, which, though very sarcastic in their style, were morally sound. He also tried his hand at novel-writing, and Shakespere was indebted to his "Apollonius and Sylla" for the principal events in "Twelfth Night."

In his pamphlet entitled "The Honestie of this Age," published in 1614,[1] he draws a sad picture of the manners and customs of the English in the beginning of the seventeenth century.

In showing up the different professions, he draws a parallel between the physicians and lawyers, considerably in favour of the former.

"They say it is an argument of a licentious commonwealth where phisitians and lawyers have too great commings in, but it is the surfeits of peace that bringeth in the phisitian's gaine; yet in him there is some dispatch of businesse, for if he cannot speedily cure you, he will yet quickly kill you; but with the lawyer there is no such expedition; he is all for delay, and if his tongue be not well typt with gold, he is so dull of language, that you shall not heare a comfortable worde come out of his mouth in a whole Michaelmasse tearme."

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[1] The Honestie of this Age. By Barnaby Rich, Gentleman, servant to King James I. With an Introduction and Notes by Peter Cunningham. Lond., 1844.

Of men he says: "They doe use religion as women doe use their paynting stuffe, it serves but to cover their deformities; they have one conscience for the church, another for the market; and so they keepe a good one for Sundayes, it makes no matter for all the weeke after."

After narrating how some women went to church to see the fashions, or to show themselves off, he tells a story of one of them worthy of quotation. She had heard the preacher discoursing of the great day of judgment, and when she went home she burst into a sudden fit of weeping, and was asked the cause.

"Her answere was that she was now but come from a sermon, that had so troubled her in her thoughts, that she could not refraine from shedding of tears. Those that heard her, thinking that shee had been stricken with some godly remorse in remembrance of her former misled life, beganne to comfort her, telling her how God had mercy in store for all penitent sinners; and her tears so distilled, being an argument of her heartie repentance, there was no doubt in her but to hope of salvation. 'Alas!' said shee, 'it is not the remembrance of my sins that doth thus perplexe me; but when I consider with myselfe, what a great assembly will then make their appearance at the day of that generall sessions, which the preacher spake of, it maketh me weepe to thinke, howe ashamed I shall be to stand starke naked before such a presence (as he saies) will be then in place.'"

The amusing scene in the second part of "Henry IV.," where Falstaff and Shallow select recruits from a very tattered lot, Falstaff letting off two of them on being bribed with £3, is evidently not such an overdrawn picture as we might have supposed; for Barnaby Rich, a few years before Shakespere wrote that play, published a treatise on "The Maner of Chosing Souldjers in England." In it he says: "The pety constable, when he perceyveth that wars are in hand, foreseeing the toyles, the infinite perilles, and troublesome travayles, that is incident to souldjers, is loth that anye honest man, through his procurement, should hazarde himselfe among so many daungers, wherfore if within his office there hap to remayne any idle felow, some drunkerd, or seditiouse quariler, a privye picker, or such a one as hath some skill in stealing of a goose, these shall be presented to the servyce of the prince; and what servyce is to bee loked for amongst such fellowes I thinke may easily be deemed."

In a letter from Barnabe Googe to Rich he writes:—

"That noble gentleman, Sir William Drurie, a paragon of armes, at this day was wont (I remember) to say that the souldiers of England had alwayes one of these three ends to looke for—to be slaine, to begge, or to be hanged."

His anecdote about tobacco, which had been very lately introduced, is quaintly told:—

"I remember a pretty jest of tobacco, that was this. A certain Welchman comming newly to London, and beholding one to take tobacco, never seeing the like before, and not knowing the manner of it, but perceiving him vent smoake so fast, and supposing his inward parts to be on fire, cried out, 'O Jhesu, Jhesu! man, for the passion of Cod, hold! for by Cod's plud ty snowt's on fire!' and having a bowle of beere in his hand, threw it at the other's face to quench his smoking nose."

Though he lived a good deal later in the seventeenth century than Shakespere, I do not think this essay would be complete without making mention of Gideon Harvey, M.D., "their Majesties' Physician of the Tower, and Fellow of the Colledge of Physicians of the Hague," who wrote a curious little book entitled "The Art of Curing Diseases by Expectation." This book is full of caustic humour, cutting up the practice of the physicians of the period, and showing that the so-called cures were not the result of the numerous remedies employed, but the natural termination of disease. He divides medical men into six classes—1. The ferrier or farrier doctors, prescribers of iron; 2. The ass doctors, dealers in ass's milk; 3. The Jesuitical doctors, who employed Jesuit's or cinchona powder; 4. The Dullhead physicians, who sent their patients to drink the waters of Tunbridge or Dulledge; 5. The butcher doctors, who were addicted to bleeding; and, 6. The purgative doctors,[1] "who aver that all distempers are to be expelled at the fundament."

He sums up his opinion of them in these words:—"To speak the truth, they are much short of the candor, honesty, modesty, learning, and industry the antients used, in making their observation on diseases and remedies, which was such, that they found that abstinence and lying still (that is, doing nothing and being spectators of nature), cured more distempers than all their interruption by physick." Here is his opinion of the scientific investigators of his day, and more especially of Harvey, his illustrious namesake, whom, I am sorry to say, he seems to have held specially in contempt:—"From being a little versed in the silly *methodus medendi,* and now and then ripping up the body of a malefactor in public, and in their capacity of prating of monstrous pretences, and vain discoveries in phylosophy and physick, they infer themselves absolute professors of their art; whereas a sea-horse in the bleeding himself, a dog in eating grass, a crane in squirting salt water into his fundament for a clyster, must by them be acknowledg'd for their masters, to whom, as their scholars, they are indebted for part of their practical physick."[2]

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[1] In original, T——d-doctors.

[2] This is almost a literal translation of Pliny. (Historia Naturalis, Lib. VIII., Cap. XL. et XLI.)

He mentions two consultation cases that made a great noise at the time, in which Harvey was wrong in diagnosis. The first, a Mr Farwel, barrister of the Temple, in which "Dr Harvey ingrossed to himself the speaking part, by reason of his extraordinary claim to anatomy." He said the disease was aneurism of an artery, too remote to come at, and therefore incurable. The patient "removed his corpus cum causa to Chelsie, where Sir Theodore Mayerne lay bedridden at his country house, who told him he could cure him by throwing the disease for a short time into either his arms or his legs, which he could best spare." "Mr Farwel said, his business being sedentary, he could best yield to the captivity of his legs, though even they, on the doctor's assurance, should be released by a month or six weeks' diligence at the bath."

The disease proved to be "an obstinate obstruction of the glanduls of the mesentery," which pressed on the arteria magna, and the patient was cured in the time specified.

The second case was a tailor in Fleet Street, who had sciatica, and Harvey would not engage in the case without a distinct answer to three points—"1. Whether he could sequester himself from his trade for three months. 2. Whether he valued the expense of fifty pounds beyond the recovery of his health. 3. Whether he could contemperate his passion in enduring the part to be laid open to the bone, by cutting or burning. The patient very readily consented to the two former conditions, time and money; but to the third being entirely averse, took his leave with the ceremony due to so famed a physician, and applied himself to another of a much lower form, who with little preamble advised him to the bath, where he received a complete cure in six weeks." His animus against Harvey comes to a climax in the passage where he compares speculators in anatomy to the neglect of pharmacy, to bricklayers or stonecutters studying the philosophy of stones instead of indulging in practical work. He says: "An instance whereof I will give you, in one that was the greatest anatomist of his time, and no extraordinary physician, namely Dr William Harvey, whose erroneous judgment was very remarkable in the prescription of a purge for Esq. Rainton of Enfield, where the apothecary, refraining to prepare more than half the proportion, notwithstanding gave him fourscore stools, which otherwise, according to the doctor's measures, must unavoidably have scoured him from the close-stool into the other world."

And now, Mr President and Gentlemen, I must conclude this rambling discourse, in which I have not attempted to philosophize, or even point a moral, but rather by a series of quotations from many and varied sources, to give you a medical aspect of Shakespere and his times.

ARTICLE II.—*Note on Intrauterine Craniometry.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Obstetrical Society of Edinburgh*, 14*th April.*)

THE subject of intrauterine craniometry, or measuring the dimensions of the fœtal head before birth, is surely destined to assume great importance, on account of its affording the accoucheur information of the highest value to guide him in the treatment of labour rendered difficult by deformities of the pelvis, or other cause of contraction of the genital passage, or by enlargement of the head.

It is several years since I called the attention of the profession to the remarkable neglect of intrauterine craniometry, while the closely correlated procedure of pelvimetry was, and had long been, the subject of much investigation and experiment, and that not without valuable results for practice.

To show the practical importance of intrauterine craniometry is so evidently a work of supererogation that I shall not enter upon it. It is the natural and now desiderated complement of pelvimetry; each, in any individual case, adding importance and interest to the other. And it is the more desiderated on account of the recognised imperfection of our best pelvimetry, an imperfection which is truly reflected in the well-known apophthegm of Dubois and of Credé on the propriety of waiting, especially in first deliveries, to see what the natural forces of labour can effect, even in a considerably contracted pelvis; and which is illustrated by the occasional occurrence, in the hands of learned and experienced men, of spontaneous delivery when even cæsarean section had been expected and prepared for.

My object in this note is merely to indicate the lines of inquiry already occupied, or that should be taken up, with a view to the advancement of this matter. These lines of inquiry either lead, or have already led, to valuable results, which have a bearing more or less direct on the great object of telling the dimensions of the head of a child before it is born. But it is important to remark that mere dimensions are not all that the accoucheur desiderates, and it is to mere dimensions that I am now directing attention.

Another important quality, for the obstetrician, of the fœtal head s its mouldability, a quality in which fœtal heads differ greatly. The difference is not only in degree of mouldability, but in kind, and also under pressure on different parts or in different directions. Any one who has made experiments with a view to the study of the omparative merits of version and the long forceps, will recognise he interest attaching to these qualities of the fœtal head.

In stating the subject of intrauterine craniometry, I shall first llude to those inquiries from which only a little aid is to be expected, and then advance to those which are more or less certain of being fruitful in the desired way.

We must not despise indications of the most trifling value, such as are to be derived from observation of—

1. *The race to which the parents belong.*
2. *The stock or breed from which they spring.*
3. *The size of the individual parents, and especially of their heads.*

Much interesting knowledge is, it is well known, already accumulated as to the cranial capacity of different races of men. But, so far as I know, the observations have been confined to adults; and, while these are, no doubt, of value in reference to the matter on hand, it would be more important for us to have measurements of the fœtal heads of different races. I may remark that we already possess many contributions to the healthy pelvimetry of different races.

The influence of the stock or breed from which the parents are sprung, and of the size of the individual parents, and especially of their heads, is matter of general knowledge. This kind of influence has been hitherto clearly observed only in some of the lower animals, in which there are more remarkable variations according to breed and in individuals than in the human race, and in which the observations have included both parents. Indeed, although few would express any doubt as to the influence of stock or breed or family, and of individual parents, yet the subject cannot be said to have been in any degree scientifically examined. There can, however, be no doubt that it is well worthy of careful scrutiny.

Gassner and Frankenhaeuser have indeed shown the very interesting direct relation between the weight of the mother, the size of the mother, and the weight and size of the fœtus; but their observations need to be corrected or added to by taking into consideration the male as well as the female parent, and the former has hitherto been altogether omitted from the inquiry.

The next points to which attention must be paid are,—

4. *The age of the parents;* and,
5. *The number of the pregnancy.*

On these matters it is well known that much labour has been bestowed by Hecker, Veit, Frankenhaeuser, Castell, Wernich, Cohnstein, Schroeder, and others; the general opinion being, that weight and length of the child and size of the head increase with the age of the mother, and with the number of the pregnancy; but I have never formally disavowed my own opinion as to a climax and anti-climax in the measurements of the fœtus, rising from immaturity to maturity of the mother, and falling from maturity to elderliness, and rising from a first pregnancy to a higher number, and then falling off with an excessive number.

We now come to a means of arguing as to cranial measurements, which is familiar to all, viz.,—

6. *The period of pregnancy.*

Every one is familiar with the anatomical basis of the practice of induction of premature labour, and with the actual measurements by

Figueira and many others of the sizes of fœtal heads, and especially of the most important bi-parietal diameter, at different periods of pregnancy.

7. *The sex of the fœtus.*

This can seldom be made out before its birth, even during labour. In the rare cases when it can be so, its significance will be easily appreciated. Every one knows the measurements and observations of Clarke, Quetelet, Riecke, Simpson, and their followers, which show the greater size of the male than of the female head, and the greater difficulty and danger of its birth.

The value of the rate of the fœtal heart's pulsations, as pointed out by Steinbach, Frankenhaeuser, and Cumming, must not be altogether forgotten. So far as the rate can be held to indicate the sex, it may be held as contributing to our information under this head.

We reach now methods of more exactness, which have only recently been introduced, and which have still to be perfected. Even as they at present stand, however, in their youthful feebleness, they are far superior to the presumptuous method which we often follow, and which is truly only the rash guessing of men not so ignorant as to be insensible of the advantage of accurate knowledge.

8. *The size or length of the fœtus.*

The recent works of Ahlfeld and Sutugin show that the length of the fœtus can be, with considerable accuracy, made out during pregnancy. The researches of Hecker, Schroeder, and Ahlfeld show, that the length of the fœtus is a better criterion of its development than its weight. Hecker, Ahlfeld, Pfannkuch, Sutugin, and lastly Fehling, have shown that the development of the head, as measured by its horizontal circumference, increases in some kind of direct ratio with that of the body. The length of the fœtus is thus shown to give us a means of estimating the size and development of its head. To this deduction, there is, as Fehling points out and illustrates by example, a remarkable exception in the case of twins.

9. *The size of the anterior fontanelle.*

This can also be ascertained occasionally in an uncertain way during pregnancy, through a thin lower uterine segment, and, at any rate, in the early part of labour. Now, Fehling has recently shown in an admirable paper, that this fontanelle increases in size during the latter part of pregnancy, the part in which the operating obstetrician is most interested; and he points out that its size may be used as an indication of the general fœtal development, its smallness as an indication of the opposite. His interesting observations form a remarkable addition to those of Elsaesser, on the increase of this fontanelle for some months after birth.

10. *The length of the sagittal suture.*

The importance of this has been pointed out by Schroeder; but, so far as I know, he has made no measurements with a view to its being utilized in practice. A great improvement is made in this measurement by Fehling, who calls it the distance between the anterior and posterior fontanelles, and takes it from the upper angle of the posterior fontanelle to a line crossing the anterior fontanelle, and joining the two coronal sutures. Fehling's observations show a kind of direct relation between this dimension and the length of the child.

11. *Measurement of the fœtal head through the uterus and abdominal wall.*

This is a plan which I have attempted, but without sufficient repetition and care to enable me to say what value it may possess. I trust, however, to be able, ere long, to lay before the Society careful observations made to determine this point. It is a matter of course that such observations will be easier carried out in advanced pregnancy in cases of contracted pelvis, when it is most desiderated, than in ordinary pregnancies, especially the first, in which the head sinks deeply into the bony cavity. In these latter, however, some combination of internal and external measurements may be available.

In cases of hydrocephalus the value of this plan is well known, and has no doubt been realized by practitioners who have met with these not very rare cases.

12. *Measurement by the forceps.*

It is easy to remark that the blades of the forceps will be more separated in proportion as the head is large, and that the handles are separated exactly in proportion to the distance between the blades. As the practitioner can determine what diameter of the head he seizes, and how much the head occupies the belly of the instrument, he can by their application determine the size of a diameter of the head, and by reapplications he may arrive at the determination of other measurements. I have often received good indications from the plan, and in one case was led by it to recognise hydrocephalus; and in a report of a lecture by Carl Braun, I find he has availed himself of this expedient to measure a hydrocephalic head.

It is at once evident which of these plans are available during pregnancy, which during labour, and which in both states. It is also evident that they are all as yet very imperfect and little reliable resources, whether adopted singly or in combination. It is in the latter way, no doubt, that the practitioner should proceed.

Investigators, however, should not be discouraged, for they may reflect on the imperfect and unsatisfactory character of pelvimetry, even now after so much learning and ingenuity have been expended upon it. Practitioners, also, will not despise the small beginnings, for they will be glad to have the value and precision of pelvimetry ked out by the use of intrauterine craniometry.

ARTICLE III.—*Notes on "Changed Aloin" and the Resin of Aloes.* By WILLIAM CRAIG, M.D., C.M., F.R.S.E., F.R.C.S.E., Lecturer on Materia Medica, etc., Edinburgh School of Medicine.

PART II.—RESIN OF ALOES.

IN a previous paper I gave an account of some researches into the action of changed aloin. Aloin is the most important constituent of aloes, inasmuch as it has been clearly shown to be the active principle of the drug. It occurs in beautiful crystals, which are supposed to be a hydrid of the amorphous aloin. It constitutes about 25 per cent. of aloes. It gives no ash when burned. In addition to aloin, the Messrs Smith of this city have recently succeeded in obtaining from aloes a volatile oil, which exists in very small quantity in the drug, only one ounce being obtained by them from 400 lbs. of aloes. This oil has a resemblance in taste and smell to oil of peppermint. To the presence of this oil is due the odour of aloes.

Another important constituent of aloes is the *resin of aloes*, the substance which is deposited when a decoction of aloes cools. This *resin* often contains a variable amount of aloin, on account of the extreme difficulty of entirely exhausting aloes of its aloin. The Messrs Smith were kind enough to prepare for me some of this resin with very great care, so as to obtain the resin in a pure state, and as free from aloin as any resin can well be. They also furnished me with the following particulars regarding experiments made on this resin:—

They took 883 grains of resin, thoroughly dried, and found that of this quantity 753 grains were soluble in rectified spirit, and 130 grains were insoluble. The *soluble resin* yielded one per cent. of ash when burned. The *insoluble resin* gave 23 per cent. of ash.

Very peculiar views regarding this *resin* are held by Dr Tilden and others regarding its physiological action, and in most works on materia medica it is spoken of as *modified aloin*, as aloin oxidized or otherwise changed by exposure to the air. The experiments above related are sufficient to disprove the common opinion that this resin is modified aloin. Chemically, it is essentially different from aloin, and physiologically it bears no resemblance to it whatever. Were it *changed aloin*, it would produce no ash when burned, and, moreover, we would expect it to have the physiological action of aloin, which "changed aloin" undoubtedly has, as I have demonstrated in the first part of my paper.

I come now to consider the action of this *resin of aloes*, a substance regarding whose chemical composition there is much diversity of opinion, and whose physiological action has hitherto never been fully investigated. The fact that this resin often contains a variable amount of aloin, has led some observers to the conclusion that the resin of aloes possessed purgative properties. With th determining this important question, I made the following experiments. These were performed on healthy young rabbits.

For six days I gave a rabbit daily 14 ounces of carrot, feeding it always at the same hour. The urine and fæces were carefully collected, and their amount duly ascertained. The following Table represents the results:—

TABLE III.

No Injection.

1st day,	urine	86	fluid drachms,	fæces	209	grains.
2d	„	82	„	„	167	„
3d	„	73	„	„	3	„
4th	„	78	„	„	341	„
5th	„	82	„	„	0	„
6th	„	64	„	„	70	„
Average,	„	77·5	„	„	136·6	„

During the following six days, I gave the rabbit daily, at the same hour, 14 ounces of carrot, and injected subcutaneously one grain of the "resin of aloes" in solution. The results are given in the following Table:—

TABLE IV.

One Grain of Resin of Aloes Injected Daily.

1st day,	urine	96	fluid drachms,	fæces	6	grains.
2d	„	60	„	„	83	„
3d	„	76	„	„	213	„
4th	„	84	„	„	100	„
5th	„	60	„	„	290	„
6th	„	100	„	„	105	„
Average,	„	79·3	„	„	132·8	„

On comparing these two tables, we find that during the second six days the urine was slightly increased, but not to such an extent as to warrant the conclusion that the resin of aloes is possessed of diuretic properties. With regard to the fæces, it will be observed that the amount varied much on different days; from nothing to 341 grains during the first six days, when no injection was given, and from 6 grains to 290 grains during the time of the injection. The daily average was nearly the same in both cases, being 136·6 grains when no injection was used, and 132·8 grains when one grain of the resin of aloes was injected daily. In this case the fæces were slightly diminished during the time of the injection, clearly proving that one grain of the resin of aloes failed to produce any purgative action.

Being anxious to ascertain if larger doses of the resin of aloes would produce any different results, I accordingly injected subcutaneously two grains of the resin daily, and fed the animal as before with 14 ounces of carrot daily. The results are given in the following Table:—

TABLE V.

Two Grains of Resin of Aloes Injected Daily.

1st day,	urine	72	fluid drachms,	fæces	202	grains.
2d	"	80	"	"	16	"
3d	"	76	"	"	200	"
4th	"	78	"	"	190	"
5th	"	72	"	"	771	"
6th	"	70	"	"	460	"
Average,	"	74·6	"	"	306·5	"

It will be seen at once that the urine remained much the same throughout the whole of these experiments, a result entirely at variance with the opinion of those who believe in the diuretic properties of the resin of aloes. It was very different, however, with the fæces. During the time that two grains of the resin were injected daily, the fæces varied from 16 grains to 771 grains—average 306 5 grains; whilst the average only amounted to 136·6 grains when no injection was used, and to 132·8 grains when one grain of resin was injected daily. On examining carefully the results of the last six days, it will be observed that the large daily average is due entirely to the large amount of fæces obtained on the fifth and sixth days, but on five of the six days was the daily average above the average when no injection was used and when one grain of the resin was injected daily. There can be no doubt, then, but the resin of aloes which I used acted as a purgative in doses of two grains, though in doses of one grain it failed to produce any purgative action.

With the view of ascertaining the action of this resin on man, I had pills prepared with extract of gentian, each pill containing two grains of the resin of aloes, and found that four of these pills produced three or four watery stools, again confirming by clinical observation the conclusion to which I had come by experiments on the lower animals. On communicating the results of my experiments to the Messrs Smith, they suggested that it was possible that the resin of aloes which they gave me might have contained a small quantity of aloin, and the purgative effect might have been due to this aloin. The resin of aloes with which I was experimenting was very carefully prepared from a solution of aloes in boiling water, and had received four additional washings. The Messrs Smith informed me that resin prepared even with such care might contain a small amount of aloin, owing to the difficulty of thoroughly exhausting aloes of its active principle. They accordingly took a quantity of the resin of aloes prepared in the manner above described, and dissolved it in rectified spirit, which dissolves the aloin as well as the resin, and to this they added boiling water and then agitated the mixture so as to form an emulsion from which the resin of aloes afterwards separated in a very fine state. The resin so prepared may be regarded as pure resin and as free from aloin as any resin can well be.

The following experiments were performed with the resin prepared in the manner just described, and were conducted in much the same way as were my previous experiments.

A healthy young rabbit weighing 39 ounces, was fed daily at a fixed hour with 14 ounces of carrot for six days, and the urine was carefully measured and the fæces weighed every day. The results are given in the following Table :—

TABLE VI.

No Injection.

1st day,	urine	56	fluid drachms,	fæces	4	grains.
2d	"	76	"	"	30	"
3d	"	88	"	"	2	"
4th	"	80	"	"	203	"
5th	"	64	"	"	67	"
6th	"	64	"	"	57	"
Average,	"	71·3	"	"	60·5	"

During the next six days I fed the rabbit on the same diet and in the same manner, and injected subcutaneously two grains of resin of aloes. The first two injections were with the resin I first used, the others with the new resin prepared with extra care.

The results are given in the following Table :—

TABLE VII.

Two Grains of Resin of Aloes Injected Daily.

1st day,	urine	68	fluid drachms,	fæces	127	grains.
2d	"	56	"	"	192	"
3d	"	56	"	"	87	"
4th	"	64	"	"	30	"
5th	"	48	"	"	3	"
6th	"	66	"	"	232	"
Average,	"	59·6	"	"	111·8	"

On comparing these two sets of experiments it will be seen that during the second six days the urine was slightly diminished, but the fæces were considerably increased. The average when no injection was used was 60·5 grains daily, whereas the average rose to 111·8 grains when two grains of the resin of aloes were injected. It must be remembered that the first two injections were with the resin I had formerly used, and which was found in doses of two grains to be slightly aperient, as shown in Table V. The average for these two days was 159·5 grains, but only 88 grains for the other four days.

Having found that 14 ounces of carrot were more than the rabbit could consume, I fed it during other six days on 11 ounces of carrot daily. During the six days I used no injection, being anxious to learn if discontinuing the administration of the resin of aloes would in any way affect the amount of the fæces.

The results are continued in the following Table :—

TABLE VIII.

No Injection.

	urine		fæces	
1st day,	urine 36	fluid drachms,	fæces 2	grains.
2d	76	”	81	”
3d	64	”	198	”
4th	52	”	420	”
5th	56	”	0	”
6th	68	”	120	”
Average,	54·6	”	136·8	”

During the next six days I fed the rabbit also on 11 ounces of carrot daily, and injected each day two grains of the *new resin* of aloes which was so carefully prepared.

The results are given in the following Table :—

TABLE IX.

Two Grains of Resin of Aloes Injected Daily.

	urine		fæces	
1st day,	urine 44	fluid drachms,	fæces 154	grains.
2d	56	”	197	”
3d	64	”	94	”
4th	80	”	50	”
5th	52	”	272	”
6th	60	”	108	”
Average,	59·3	”	145·8	”

On comparing the last four tables, it will be seen that the daily average quantity of urine was greatest during the time when no njection was used, a fact totally at variance with the opinion of hose who hold that the resin of aloes is diuretic. With regard to the fæces it will be observed, that the average amount of fæces during the last six days exceeded that of the previous six days by 9 grains, an increase so small that we cannot draw from it any conclusion in regard to the purgative property of the resin of aloes. The largest amount of fæces obtained during one day, when the njection was used, was 272 grains; whereas, on the other hand, 120 grains were obtained one day when no injection was given. The daily average increased during each successive period of six days, without reference to the injection of the resin of aloes. Durng those twenty-four days the rabbit increased in weight from 39 to 43½ ounces. On a careful comparison of these experiments with the resin of aloes, it will be observed that no appreciable effect was produced, even by the subcutaneous injection of two grains of his resin.

During the first six days, when no injection was used, the daily average amount of fæces was 60·5 grains; during the second six days it was 111·8 grains, with two grains of resin of aloes injected daily; during the third six days, with no injection, the daily average was 136·8 grains; and during the last six days, with two grains of resin of aloes injected daily, the average was 145·8 grains. There is but one conclusion to be drawn from these experiments, namely, that the resin of aloes is not purgative, and cannot be the active principle of the drug.

I had some of this resin made into pills, with extract of gentian, each pill containing two grains of resin, and found that six such pills failed to produce any effect whatever. I thus found that my clinical experience fully confirmed the conclusion arrived at from my experiments on the lower animals. Dr Farre, of London, gave to patients doses of three grains of resin of aloes, and said he found it almost inert, and that it seldom produced any evacuation. He, however, holds the rather doubtful conclusion, that though it is inert when separated, it is probably active when combined. In those cases where three grains produced evacuation, the activity was most likely due to the resin containing a small quantity of aloin.

As previously stated, it is exceedingly difficult to exhaust the aloes entirely of its aloin; and in proportion as it contains any of this substance, so will it prove more or less active. This fact has been illustrated in Table V. When the resin is prepared with the care that the last sample was, with which I experimented, I believe it will prove altogether inert. Similar results were obtained by Dr Garrod, although it is evident from his experiments that the resin was not always free from aloin. He says the *extract* is far more active as a purgative than the resin, and admits that the resin often proved almost inert. Had the resin with which he experimented been entirely exhausted of aloin, it would, I believe, have proved always inert. Nevertheless, the experiments of Dr Farre and Dr Garrod are exceedingly valuable and very interesting, and tend very much to prove that the resin is an inert substance.

I have thus far established the point, that the opinion of Dr Tilden, regarding the activity of the resin of aloes, is altogether untenable. It is contradicted by the experience of the observers just named, and my experiments both on man and on the lower animals are conclusive on the point.

In speaking of the action of the resin of aloes, I must not omit to mention, that to this resin is generally ascribed the griping property of aloes. I believe that aloes does not gripe nearly so much as writers on materia medica would lead us to believe. When it does gripe it is probably on account of some impurity in the drug, and certainly it cannot by any possibility be due to the resin. It was with the view of determining this question that the experiments of Dr Farre and Dr Garrod were performed, and they both came to the same conclusion, that the resin was not the cause of the griping. My experience fully coincides with theirs. I gave it to a large number of patients, and in very different circumstances, in doses varying from 4 to 12 grains, and in no single instance did it produce griping. It thus appears that the resin is not only inert, but is also a very harmless substance.

These experiments are also interesting as bearing on the chemical constitution of the resin of aloes. The results are entirely at variance with the generally received opinion, that the *resin* is "modified aloin." I have clearly demonstrated that "changed aloin," which very much resembles *resin* in appearance, is an active aperient, possessing all the activity of aloin itself, whereas the *true resin* is altogether inert.

The following may be regarded as conclusions fairly deducible from the foregoing experiments:—

1. That aloin may, by exposure to the air, undergo considerable chemical change, without losing its physiological action as an active aperient.
2. That the *resin* of aloes, when thoroughly exhausted of aloin, possesses no purgative properties, and therefore cannot be the active principle of aloes.
3. That the resin of aloes is not the cause of the griping which sometimes follows the administration of the drug.
4. That aloin is an active aperient, and is in all likelihood *the active* principle of aloes.

When these experiments are viewed in connexion with the researches of numerous experimenters in regard to the activity of aloin, we are forced to the conclusion, that *aloin is the only active principle contained in aloes*, and is sufficient to account for all the purgative properties of that medicine. And that being the case, I cannot conclude this paper without expressing a regret, that in the "Addendum to the British Pharmacopœia of 1867," published in 1874, no mention is made of aloin. It ought to find a place amongst the medicines recognised in the British Pharmacopœia. It possesses the following advantages over the crude drug:—

1. Being uniform in strength, its dose can be more accurately determined.
2. Its dose being half a grain to one grain, it can easily be introduced into tonic pills without making such pills too large.
3. By using the active principle, we get rid of all impurities which are so apt to cause griping.

Its uniformity in strength, the smallness of its dose, and the certainty of its action, should commend it to the favourable consideration of all medical practitioners.

I have found the following an excellent pill for the constipation so common in females of a sedentary habit. I believe it possesses some advantages over the pil. aloes et ferri of the B. P.:—

℞ Aloin., gr. ss.
Ferri sulph. exsic., gr. jss.
Extract. nucis vomicæ, gr. ss.
Extract. belladonnæ, gr. ss.
Fiat pil.—One or two pills daily.

ARTICLE IV.—*On the Treatment of Certain Cases of Stricture of the Urethra by a Combination of Internal and External Division.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

(*Read before the Medico-Chirurgical Society of Edinburgh, 7th April.*)

CASES of stricture of the urethra, complicated with numerous fistulous openings, and great thickening of the perineal and scrotal tissues, are well known in the experience of practical surgeons. There is also little doubt that the recognised treatment of this condition is a free external division of the perineum and strictured portion of the urethra. The difficulty of thoroughly dividing the entire strictured portion of the canal in these cases is only recognised to its full extent by those who have performed this operation. I have witnessed this difficulty in the hands of the most skilful operators, and I have myself experienced it on several occasions. Especially is this difficulty met with when the stricture is a tight one; for the accurate division of the affected part of the canal on Mr Syme's staff, or without it, at a depth of from three to four inches (the increased depth depending on the thickening of the perineal textures), is a proceeding not easy to accomplish.

Another difficulty in such cases is the fact that a considerable extent of the urethra is often involved, or several strictures may exist, and experience shows that unless the whole urethra be properly dilated, a good result cannot be expected.

A few weeks ago a patient was admitted into my wards suffering from an aggravated form of this condition. His name was G. M. B., and he was 45 years of age. He had suffered from stricture of the urethra for four years, and about eighteen months before his admission abscesses formed in his perineum, buttocks, and scrotum, and gave rise to numerous fistulous openings in these situations. An examination determined the existence of a tight stricture of the urethra in the region of the bulb and anterior to it. The stricture was nearly two inches in length, and in addition there was a stricture, not so tight, in the anterior part of the urethra, about two inches from the meatus. There were five or six fistulous openings in the perineum, and several others in the buttocks, close to the anal orifice. The whole perineal textures were much thickened, and several hardened nodules of skin and cellular tissue were scattered through this region, resembling somewhat in appearance those occasionally met with in cancerous disease. The whole of the urine was passed through the fistulous openings. With some difficulty a No. 2 metal catheter could be passed through the stricture into the bladder.

In considering the treatment of this case, I knew that it would be necessary to freely incise the middle line of the perineum, and also to divide the strictured portion of the urethra, in order to obtain a cure of the affected parts; but I also knew from experience something of the difficulties in successfully carrying out this proceeding. I was therefore glad to avail myself of a hint which I received in reading some very interesting papers on stricture of the urethra, by Professor Otis of New York, for his suggestion seemed to me to be one likely to be very valuable in certain cases of the kind under observation. Professor Otis, having a case of stricture complicated with fistulous openings to treat, first incised the perineum, and opened the urethra externally to a slight extent, and then introduced a urethrotome, and freely divided all strictured portions of the canal.

Taking a lesson from this case of Professor Otis, I operated on my case in the following way:—

Having passed a No. 2 silver catheter through the strictures nto the bladder, I made a free incision in the middle line of the perineum, through the thickened tissues, and opened into the urethra immediately behind the point of the posterior stricture. I made no attempt to divide the stricture by this external incision, but, having removed the catheter, I introduced along the urethra nto the bladder the fine-grooved staff of a urethrotome (one after Maisonneuve's principle), and then, by passing the knife of this nstrument along the whole length of its groove, the entire strictured portions of the canal were thoroughly divided. The whole operation did not occupy more than a few minutes, and I was much impressed with the advantages as regards easy and quick performance over the method of external division as usually followed. A No. 12 catheter was readily passed into the bladder after the operation, and was retained there for twenty-four hours. The patient's progress after the operation was in every way satisfactory. The hickening of the tissues gradually but entirely disappeared, and he fistulous openings soundly closed. The urine now comes freely by the natural way, and a No. 12 bougie can be easily passed along the whole course of the urethra into the bladder. The operation wound in the perineum has nearly closed, and not more han a drop of urine passes occasionally through it.

A second case of urinary fistula was shortly after admitted into my wards, and was treated in the same way, with a like good result. The following is a brief note of this case:—

J. G., æt. 40, admitted on the 23d of March. He had suffered rom stricture of the urethra for five years, and six months before admission abscesses formed in the perineum and scrotum, and caused istulous openings in these situations. On admission there was a istulous opening in the perineum, and another immediately anteior to the scrotum, with thickening of the surrounding textures; all the urine was passed through the fistulous openings. The introduction of a bougie discovered a tight stricture immediately anteior to the scrotum, and a second one in the region of the bulb.

On the 25th, an operation similar to that performed in the first case was successfully carried out. The result of this operation was also most satisfactory, and the patient was dismissed cured on the 24th of April. A No. 12 bougie could be readily passed along the whole of the urethra into the bladder, the thickening of the tissues had entirely disappeared, and the whole of the urine passed freely by the right way.

This plan of operating appears to me to be well worthy of the notice of surgeons. I do not advocate it in cases of stricture, other than those of which the cases related are illustrative examples, but I have no hesitation in strongly recommending it in suitable cases. Internal division is becoming more and more recognised in this country as the safest and most efficient treatment of strictures of the urethra which do not yield to simple dilatation, and my extended experience of this operation during the last few years has made me a firm advocate of it in proper cases.

The combination of internal division with external division in complicated strictures such as have just been illustrated by these cases has, in my opinion, the following advantages over external division alone:—

(1.) The complete and accurate division of all strictured portions of the urethra.

(2.) Its easy accomplishment.

In cases of complicated urinary fistula, in which the stricture of the urethra is impassable to instruments, or a portion of the canal is obliterated, I should in the first instance endeavour to re-establish a passage by external division; and then, if any portion of the urethra could still be felt to be strictured, I would introduce the urethrotome, and freely divide by internal division such contracted portion.

ARTICLE V.—*Cases of Fatal Hæmorrhage after Childbirth.* By CHARLES BELL, M.D., F.R.C.P.

(Read before the Obstetrical Society of Edinburgh, 24th March.)

I WAS requested, on the 19th May 1872, to see Mrs Kerr, aged 42, a district patient of the Royal Maternity Hospital, who had been delivered of her thirteenth child at 2 P.M. On my arrival, I received the following history of her case from Mr Jeffrey, the House Surgeon. She was married in 1853, and had had twelve children at the full time, and one miscarriage at the third month. In consequence of a congenital malformation she had always refrained from employing a midwife or accoucheur in her confinements, and had always been able to deliver herself safely in all her previous labours, until the present occasion, when, after delivering the child, which was stillborn, she failed to remove the placenta, and, in endeavouring to do so, she separated the cord at its insertion. She did not apprehend any danger, however, and proceeded to wash the child; but she was soon seized with hæmorrhage, followed with syncope. A doctor was then sent for, but unfortunately he was from home. A midwife who lived in the neighbourhood was then called in, but on ascertaining the nature of the case, she, according to the custom of such persons in cases of danger, declined to interfere with it, suggesting that medical assistance should be obtained from the Maternity Hospital. This suggestion was acted upon, but the nature and urgency of the case not being stated, a pupil was sent, who, on discovering the condition of the woman, sent for the House Surgeon, who immediately sent for me, and, in the meantime, administered a large dose of turpentine, which seemed to check the hæmorrhage.

On my arrival between eleven and twelve at night, I found the poor woman in an extreme state of exhaustion, pulseless at the wrist, and quite anæmic. On examination, I found the entrance to the vagina in a great measure blocked up by two condylomatous tumours, about the size of the fist, occupying the labia. I met with considerable difficulty in passing my hand between them, and on reaching the uterus, which was quite relaxed, and the placenta partially but firmly adhering, I removed the portion that was detached; but, from the extreme exhaustion of the patient, I refrained from separating what was adherent, and ordered her to have stimulants, and a large dose of ergot. She rapidly sank, however, and died a few minutes past twelve, about ten hours after the child was born.

This case presents several points of interest, which I shall briefly refer to. In the first place, it is, so far as I know, the only fatal case of hæmorrhage on record among the patients of the Royal Maternity Hospital; and although I have seen many cases of puerperal hæmorrhage, this is the only fatal case I ever met with, having always found that the established means of treatment were quite sufficient to check the discharge; and had they been had recourse to in proper time in the present case, there is every reason to believe they might have been successful; but from the history of the case, it will appear that much valuable time was lost, and the system, being unaided, was unable to contract the uterus and entirely throw off the placenta—the only true source of safety in such a case. This case gives a striking example of what women can do in trying circumstances, especially when actuated by false shame. Had this not been the case, this unfortunate woman would have had the condylomatous tumours removed in proper time, and in all probability her life might have been preserved. This case also illustrates the custom among savage tribes, in which women not only deliver themselves, but immediately afterwards proceed to wash their children, and return to their domestic duties.

The following case fully bears out the opinion I have expressed in regard to the proper treatment not having been pursued in due time in this unfortunate case.

In the absence of Dr Keiller, I was requested to see Mrs J., who was in labour. On my arrival I found that she had just been delivered of a daughter, after a natural and rapid labour. The placenta was retained in the uterus, but it was easily removed; the binder was applied, and everything seemed satisfactory; but the womb relaxed, and profuse hæmorrhage came on, producing complete cessation of the pulse at the wrist for two hours, during which the uterus became contracted and relaxed several times, each relaxation being attended by a gush of blood. Ultimately, however, the uterus remained firmly contracted. I continued for five hours at the bedside of the patient, applying pressure on the abdomen with the one hand, and occasionally removing clots with the other, and irritating the internal surface of the uterus. At the same time the tincture of the ergot was given, along with first brandy, and afterwards port wine. On the following day she was wonderfully well, and I believe she made a good recovery; but I must refer to Dr Keiller, who then took charge of the case, for the further history of it. I am quite satisfied that had this case been treated by injection of the perchloride of iron, it would have proved fatal; for the inevitable consequence would have been, that on every contraction of the uterus a certain quantity of the styptic would be sucked in by the bloodvessels, forming embolism. If this remedy ever proved successful, it must have been by acting as an irritant, of which there are many more safe in their character, inducing the uterus to contract, or when the hæmorrhage proceeds from tearing of the cervix, which will be ascertained by the flooding continuing after the uterus has contracted.

ARTICLE VI.—*A Case of Intrauterine Fibrous Polypus.* By Dr UNDERHILL.

(*Read before the Obstetrical Society of Edinburgh, 24th March.*)

ABOUT the middle of January I was asked by a medical man to send a dispensary student to see Mrs Gannen, aged 49, who, he said, was suffering from prolapsus of the uterus. She was accordingly seen by Mr J. G. Brown, one of my students, who, on examination, found a swelling of considerable size in the vagina, and far up and behind it he could detect the lips of the os. I first saw her on 30th January, and obtained the following history:—For the past six months she has been suffering from hæmorrhages, which, during the earlier part of the period, intermitted for a few days or a week, but have latterly been nearly constant. At the New-year time, a tumour appeared at the genital orifice, which was described as smooth, rounded, and dark red; this caused her much pain and annoyance, and she could not sit down or walk without great discomfort and pain; the tumour soon went back, and a fortnight later she was seen by Mr Brown. The hæmorrhage was still coninuing, and the patient was anæmic, and exceedingly nervous. On naking an examination, I found the uterus lying unusually high n the pelvis, and protruding through the os, to an extent hardly f at all beyond the lips of the cervix, a smooth rounded tumour. The uterine sound passed in by the side of the tumour between four md five inches.

On the 6th February, the patient being put under chloroform, ssisted by Dr James Cumming, I seized the tumour in the vulsella, ind though I used considerable force, I could not bring it down low nough to expose or feel the pedicle. I accordingly introduced the vire ecraseur, and easily removed the polypus on the table. When resh it measured two and a half inches in length, three inches in ircumference, with a thin whipcord-like pedicle of nearly three nches more. The tumour is about the shape and size of a large ien's egg, and, with the pedicle described, looks like the old-ashioned oval wood pessary, with the string attached to assist re-noval. When fresh it was very soft, and felt almost as if it con-ained a fluid. Microscopical examination showed it to be a loose-issued fibroid, with numbers of what appear to be smooth muscular ibres, and numerous round cells lying among the fibres; it is, noreover, exceedingly vascular. No mucous membrane or envelope f any kind could be made out, the exterior boundary appearing to e some layers of flat epithelial cells. The bleeding stopped at nce, and the patient made an excellent recovery.

The only point which makes this case worth relating to the Society, is its history. The evidence appears to me to be quite listinct that the polypus was pushed out of the uterus, and down is far as the vulva, and was then gradually retracted till it was, at he time I saw it, drawn almost altogether within the cervix; and ndeed I thought I could detect a difference in this respect between ts position on the day I first examined it and on the date of the peration, a week later. That the whole uterus was prolapsed, as night be suggested in explanation, I think very unlikely, for the ollowing reasons:—(1.) The tumour seen at the vulva by the atient was described as a simple round smooth body; (2.) When xamined by Mr Brown a fortnight later, the body was distinctly elt to project far through the os, the edges of the lips being made ut lying higher up and behind it; (3.) At the time I saw it, the iterus, instead of being in any way prolapsed, lay very high up, ind the polypus was contained almost entirely within it; (4.) The whole uterus was so high up that at the time of the operation I had great difficulty in applying the vulsella without injury to the vagina; 5.) Though I pulled very hard, I did not succeed in dragging lown either uterus or tumour as low as the opening of the vulva.

If we look upon this, then, as a polypus which was driven down ind then retracted, what explanation can we offer of the mechan-ism by which this was effected? The uterine action would easily xplain the first part of the process, but the retraction of a tumour of this size within the uterine, or at all events cervical cavity, is much more difficult to understand. Elasticity of the pedicle it cannot be, for the pedicle is not elastic; the effect of muscular fibre in the pedicle itself is not sufficient, as it is very thin, and the amount of contraction necessary to pull back the tumour would be enormous. The efficient cause must be somehow or other in the muscular wall of the uterus, though I cannot conceive any form of uterine contraction capable of drawing a body up into its cavity,—or it must be in a suction power exerted by the uterine cavity, or in some modification of the retentive power of the abdomen.

ARTICLE VII.—*Ovariotomy beyond the Indus.* By Surgeon-Major WILLIAM CURRAN, A.M.D., Murree, Punjab, India.

As statistics bearing on the success or failure of ovariotomy and its practice abroad are now more needed at home than refinements of diagnosis or details of operative procedure, I make no excuse for offering the following statement of facts, as the result of my own personal experience on the subject; and the interest attaching to these will perhaps be enhanced by the notification that this was the first case that either I or my colleagues in the station at which it occurred had ever seen. It was also, as far as I can now learn, the first operation of the kind ever practised beyond the Indus; and though this circumstance can scarcely be said to affect the issue, inasmuch as there must be a beginning somewhere, yet is it not unworthy of being noted in connexion with the case; and I may so far fairly lay claim to be the first representative of ovarian surgery in the Peshawur valley. I am only sorry the result was not more satisfactory; this was not, however, my fault, as will, I think, appear further on; and the space at my command being limited, I will proceed at once *in medias res*, and introduce my patient without further preface.

Jahurum, a tall, lanky Mahomedan woman, of the Dhohee or Washerman caste, whose age may be roughly estimated at thirty-five or thereabouts, and who has borne no children for ten years or more, came under my notice early in September 1873, complaining of inability to carry any loads, or even to walk, without discomfort, to or from the washing tank. She assured me that her belly had been progressively increasing in size for the last three years, and when examined in the recumbent position, the abdomen protruded visibly beyond its natural outlines. The skin was tense and somewhat tympanitic; the breathing was hurried and difficult; she had a very troubled, desponding expression; and I noticed that when she stood up, she endeavoured to support her stomach with the palms of her joined hands. She was very anxious that something should be done for her; but as she was in a wretched state of health, I placed her under treatment in the verandah of an hospital, and as soon as her other surroundings would allow, I tapped her. But little or no impression was made by the trocar on the appearance or contents of the tumour, for the material that exuded from it was thick, viscous, and sticky, and its calibre was soon blocked up. A good deal of this glutinous stuff subsequently trickled away through the opening that existed in the abdomen, but very little relief was obtained; and having learned, meanwhile, from Wells's able article in Lane's Dictionary (vol. ii. p. 413), that accumulations of this kind were certain to recur, I determined to give up expectancy, and try the effect of more active measures. Aided accordingly by my colleague Dr Jackson, and by my friend Dr Sankey, and in the presence of three others of our confrères, and of almost all the senior subordinates in the garrison, I proceeded to operate by making an incision (under chloroform) four inches long from near the umbilicus in the direction of the pubis. The cyst was, after some careful dissection, easily recognised and penetrated; but we all saw at a glance, that no impression could be made upon it by means of the apparatus at our command, or through the wound already established; and the next step was to enlarge this. Then finding that our trocars were utterly useless, and unwilling to give up altogether at that stage, I first introduced the hollow perforated zinc leg of a bed into the cavity of the sac; and finding that this would never empty it, I next used the nozzle of a long vinegar bottle, from which the bottom had been previously removed. These were equally useless; and there being no other alternative, I enlarged the opening in the sac, and holding the lips of the wound apart with the left fingers, I thrust my right hand boldly into the body of the cyst; I then crushed smaller cysts, broke down the trabeculæ, and scooped out a quantity of viscid glutinous matter, membranes, and other debris, for which there could under the circumstances be no other mode of discharge or exit. Having completed this fatiguing process, with the aid of the kind friends already mentioned, and without, as I believe, allowing any escape into the cavity of the peritoneum, I drew out the cyst, and found, to my surprise and disgust, that it had no pedicle. This was a contingency which none of us contemplated; the work was but half done; the woman was becoming faint, and I began to fear we might not be able to finish our task. Fortunately, I had secured, as if for this very complication, a castrating clamp from a friendly "Vet." in the station a day or so previously, and with this we determined to enclose as much of the mass as we could, and to ligature or cut off the remainder. This was done with the least available delay; the abdominal wound was closed with silk and metallic sutures, and the actual cautery was freely applied to the stump. The woman recovered well from the effects of the anæsthesia and the shock, and, to obviate all risk of displacement or hæmorrhage, she was allowed to remain reclining on the same couch, in an open, airy verandah, for some two or three hours after operation.

Her subsequent progress did not keep pace with these appearances, or realize our anticipations; for though she freely partook of beef-tea and cooling drinks during the day, she did not thoroughly rally at any time, and there was an air of unrest and disquietude about her which augured unfavourably for the future. She did not sleep well the following night; the pulse ran up to 110-20, and she occasionally rejected the nourishment we allowed her. The pulse was very quick, small and feeble at the hour of morning visit, and she was evidently weaker than before, yet was there no complaint of pain; she conversed freely, though with more appearance of effort than heretofore, and the stump was healthy, and free from unpleasant odour. These negative appearances did not last long; exhaustion increased as the day advanced, and, retaining consciousness to the last, she expired painlessly and rather suddenly, about 11½ A.M., or some thirty hours after operation.

On examination of the parts a very short time after death, the uterus was found to be hypertrophied on the left side, and enlarged both as to cavity and consistence throughout. The tissue had undergone white fibrous degeneration, and it was so closely adherent, nay so continuous on the part of its base, with the body of the cyst, as to be quite indistinguishable from that structure. The left ovary had lost all trace of its ordinary flattened ovoid shape; it had a hard gristly feel, and a rough nodular exterior, and when opened, its surfaces gave evidence of a fibro-cartilaginous transformation. The right ovary was absent, or so far involved in the folds of the broad ligament of the same side as to be inseparable from them; and these latter had contracted such adhesions to the neighbouring pelvic bones and viscera as precluded separation during life.

Remarks.—The details given above are necessarily compressed to the utmost, to save time and space; but they suggest an interesting point or two of practice, and to these I now purpose confining myself. As already intimated, I have not myself, neither has any of the gentlemen who were present, seen or heard of an instance precisely similar to the one under review, and I can find no parallel to it in the few books bearing on the subject that are now at my command. It is true Mr Wells says (*op. cit.*, vol. ii. p. 415), that "firm, extensive, and vascular adhesions may sometimes render the completion of the operation impossible, but the extent of adhesions alone forms no contraindication to the operation, as far as they can be ascertained, before the operation is commenced, *except such close adhesions within the pelvis as would render separation of the cyst from uterus, bladder, or rectum extremely difficult and hazardous;*" and I may add, that I took the earliest opportunity of ascertaining, in accordance with his advice to that effect, that no such adhesions as those contemplated in the first part of the above paragraph existed in this case. How those which I have italicized could be ascertained before the abdomen is opened and the cyst tapped, does not appear, yet are they, judging from analogy and the anatomy of the parts, very likely to occur; and, as will be seen from the history prefixed to these remarks, they were the means of defeating me at the very moment in which success seemed certain. Had I not had the good fortune of having a large veterinary clamp by me at the time, I would have been obliged, *nolens volens*, to return the empty bag into the cavity of the abdomen; as it was, I acted on the promptings of the moment, and on the advice of my colleagues; and I should think that the rule of surgery (see South's Chelius, vol. ii. pp. 40, 41, etc.) which prescribes the removal of an irreducible mass of omentum, would also apply in cases similar to that so feebly described above. Upon this point, however, I am unable, for the reasons already given, to offer any decided opinion; and were this kind of interference again required at my hands, I should naturally defer in such a contingency to the advice of those who have more knowledge of these cases than I or my military brethren can pretend to. This I would fain solicit through the medium of this communication; and with regard to further experience in this province on the subject generally, I have heard of four other instances in which an operation was performed for the removal of a diseased ovary. In one of these, a European, a large collection of hydatids of the womb was removed, and the patient succumbed to exhaustion some days subsequently. In the three others, who were natives, incurable disease of the liver, or extent of adhesions, precluded further proceedings in two, and both these women recovered. In the third, a dropsical ovary was successfully removed; and this constitutes the sum of my experience of ovariotomy on either side of the Indus.

ARTICLE VIII.—*Glimpses at the Health Resorts of the Bay of Naples in Ancient and Modern Times.* By JOHN MACPHERSON, M.D., Inspector-General of Hospitals (retired).

(*Continued from page* 819.)

WE may next proceed to Baiæ proper, and speak a little more specially of its climate and of its remains. Baiæ can never have been a very bracing spot, especially as hills shut out the influence of the Favonius or west wind, which the open shores of the Mediterranean enjoyed. Baiæ was undoubtedly occasionally unhealthy in the days of Cicero; for in a letter to Dolabella—which, by the way, showed that the Romans visited it as early in the year as April—he congratulates him on Baiæ having suddenly become healthy for his sake. Still it is impossible that the place should have been very insalubrious, and yet have remained for ages as popular. It is since the formation of Monte Nuovo that Baiæ has got its evil reputation. An Italian Jesuit, Camillus Eucharius de Quintas, who wrote an elaborate poem on the island of Ischia in Latin hexameters about 150 years ago, considers Cumæ and L. Acherusia unhealthy, and insists on the deadliness of the shores of Baiæ, saying that they must not be visited until the sun is well up in the heavens. Avernus, he said, was healthy.

The mineral waters at Baiæ were very numerous. From Tripergula to a mineral water in Misenum, Alcadinus enumerated no fewer than twenty-two springs. These included one of petroleum, and the baths of St Lucia, endowed with very wonderful properties, equal even to giving sight to the blind, and said to be so frequented by strangers, that the Neapolitans could find no room for themselves at them.

Bartolo, in 1688, strange to say, found nearly as many springs to describe as Alcadinus, some of them restored by the munificence of Charles II. of Naples. Even in the "Guide to Pozzuoli and its Neighbourhood," published in 1768, ten baths are spoken of as being in existence. Of all these baths, only one, or perhaps, if we count the bath of Tritoli, two, are now in occasional use. The chief one is the Sudatorium of Tritoli, which in modern times commonly passes by the name of the Stufe di Nerone. The distich of Martial—

> "Quid Nerone pejus?
> Quid thermis melius Neronianis?"—

is usually applied to this place, but was really meant to apply to the magnificent thermæ which he built in Rome. I see no evidence of his having built any baths here. He did indeed inaugurate the mad scheme, of making all the hot springs along the coast from Avernus to Misenum discharge themselves into one huge covered piscina, extending from the one place to the other—a scheme, the execution of which was commenced, but was never carried out. The stufe of Tritoli have been considered to be the *myrteta* so often spoken of by authors. We have but a very slight description of the myrteta in Celsus, who uses these words, "Where hot vapour issuing from the soil is enclosed in an edifice, as in the myrteta above Baiæ." Possibly the situation of these stufe may in some degree correspond with the description "above Baiæ," but I should rather have expected to find them in the hills behind and to the west of Baiæ. There is no trace of such an edifice as is alluded to by Celsus, or as would have suited the requirements of the luxurious Romans. The long passages and heated chambers, and rude arrangements of these stufe, are in part threaded by most strangers. All its windings were made out, and a plan of them drawn by Signor Bulifon about 1700, not without *incredibili sudori*. They are most powerful sudorifics, and are used to a considerable extent by the eople of the country. No longer than a hundred years ago, about

00 patients used to be annually sent to them from the hospitals of Naples in the month of June.

Of remains of establishments along the coast, the most extensive nd best preserved, the *piscina mirabilis* at Baoli, appears not to ave been a swimming-bath, but a reservoir of water for the fleet. The ruined buildings at Baiæ, now passing by the names of the emples of Venus, of Mercury, and Diana, were evidently baths, ike the remains of similar constructions at Tritoli below the stufe. About all these buildings there are remains of water channels—ndeed, springs were to be found in most of them a hundred years go.

The waters of Baiæ were often spoken of by the ancients as sulphurus, and no doubt some of them were so; but they were essentially like hose of Puteoli—warm waters containing common salt and a little oda. As already said, most of them have been lost, but they could o doubt be found again, if they were wanted—of which being the ase there seems to be no probability. The only traces of the Baiæ springs now commonly known, are the hot waters in the Tritoli stufe above mentioned, in which it is usual to boil eggs or the delectation of travellers, and the lukewarm waters in what s commonly called the Sibyl's Cave, to the south of Avernus. There is also a mineral water called the Capona, in a so-called emple on the east side of Avernus, and no doubt near it might be e-discovered the old chalybeate spring, or Balneum Ferri. Even n the time of the early emperors all the wood about Avernus had een cut down; it had lost most of its traditional gloom, and he neighbourhood had become healthy; and it has not in modern ays had the evil reputation for fever, which attaches to Baiæ.

Thus far we have had to treat chiefly of waters historically nteresting, but of little practical value at present, as they and their ses have been forgotten. Henceforth, commencing with Pozzuoli, e have to do with waters, some of which are used very extensively; ut before reaching that place, we must pass the site and the meagre uins of the villa of Cicero and its Academia, with portico and rove. It is only their association with the past that gives them n interest to us. The painstaking but somewhat dull poet, hough valuable for his account of the topography of the neighbourhood, Silius Italicus, lived in Cicero's villa; and according, at east, to one account, the remains of the Emperor Hadrian found heir first resting-place in its grounds, and a temple was built over hem. But the Balneum Pratior of Cicero is interesting to us, because it sprang up in his garden not long after his death. It eems, like all the buildings of the place, to have completely disppeared. It was considered a specific for weak eyes, and its virtues have been transferred to one of the springs of the temple of Serapis. It is curious to observe how very highly Pliny thought f some not very brilliant verses by a freedman of Cicero on this pring, the two last lines of which say, that Cicero had so many readers, that it was well there should be fresh waters for their eyes.

> " Ut quoniam totum legitur sine fine per orbem,
> Sint plures oculis quæ medeantur aquæ."

Coming to the next group of waters near Naples, we reach Pozzuoli, formerly Puteoli, doubtless so called for its numerous wells. They were known nearly as early as those of Baiæ; and in the old world the mineral waters and sea-bathing of Puteoli enjoyed a high repute. Of the fortunes of Puteoli it is not necessary to say much. From being a great city and a centre of commerce, it has dwindled down into a third or fourth rate town. It was nearly deserted at the time of the formation of Monte Nuovo, when most of its buildings were thrown down, but it never fell into the utter decay of Baiæ, and not being shut in from the sea breeze, it has never been so unhealthy as the former place.

The only waters of Puteoli proper, in these days, are at its west end, the baths of the temple of Serapis. There are three springs, but the one that is best known has long passed by the name of Aqua del Cantarello, or of the Tre Colonni, the pillars now of such geological fame, as bearing witness to the successive depressions and elevations of the coast. The Aqua Cantarello in former days was covered by the sea in stormy weather. The springs seem to have been upheaved during the changes of 1538. They were rediscovered in 1750, and there is now a considerable bathing establishment there, raised on the remains of the ancient thermæ. The baths are not well kept. There are thirty separate baths, and one piscina. The waters are slightly thermal, limpid, and inodorous; they contain a little soda and common salt, and are not powerful. They are used only for baths, and are found to be useful in gout and rheumatism. One of the springs, dei Lipposi, like that which sprang up in the grounds of Cicero's villa, has local fame in weak eyes.

Just above the town of Pozzuoli, it was intended to have a new establishment in the beautiful grounds of the Villa Cardito, and to open up some of the springs, which were probably those of the old Bagni Orthodonici, which used to be considered a sovereign remedy in consumption. In former days vapours of a deadly nature were said to escape from the rocks, and required caution on the part of visitors. It was also proposed to restore a large piscina, which has been preserved nearly as well as the vast one at Baoli. Owing, however, to the season of my last visit to Pozzuoli and other causes, I could obtain no information as to the progress of the undertaking. No one in Naples took any interest in it.

Before passing further along the coast to Bagnoli, it is desirable to visit the heights above Pozzuoli, the old Phlegræan fields, or Forum Vulcani, which is sterile and verdureless, as when it was described in the verse of Petronius Arbiter. It is unnecessary here to describe so well known a scene, or the decay of its manufac- e of sulphur and of alum; but one or two things must be said of ts healing virtues. Ever since the days of Galen it has enjoyed a reputation of great salubrity owing to its sulphur vapour *fumaroli*. Many a story has been told of patients trying other climates and change of air in vain, and being at last cured of consumption by a residence here. It is said that glandered horses are sometimes sent with advantage to breathe the air of the Solfatara.

There is a powerful thermal spring in the Solfatara, containing sulphate of iron and sulphuric acid, probably of much the same constitution as the waters of the Pisciarelli, which I shall presently mention. This water is described in the verse of Alcadinus. He says, if your nose only does not object to the smell, that it has marvellous virtues, curing sterility, and also clearing the eyes (in no pleasant fashion) by causing vomiting. There is a small bath-house in the Solfatara; but the waters have been chiefly used of late years as local applications in the hospitals of Naples, especially in ulcers and fistula. There is no doubt that the waters are strongly acid and styptic. If we cross the Solfatara, and descend its slope towards Lake Agnano, the Montes Leucogæi, or white earth mountains of Pliny, we come to the more important waters of Pisciarelli, or the Balneum Bullæ, so called from its high temperature and the sound of boiling. It is probable that the water contains as much as 1 grain of sulphate of iron, ½ of alum, and ½ of sulphuric acid in the oz. It has a temperature of about 156°. I did not actually visit the spot; but there is said to be a small stufa or sudatorium here, and the spring is in a small cottage, which is provided with some rough baths.

Pliny told us that these waters were good for the eyes and for wounds. To this day they are chiefly employed as external applications in eruptions and sores. However, they may also be taken nternally with care, in quantities up to five or six ounces, when they are found to be good astringents in chronic diarrhœa and in hæmorrhages. Both this and the Solfatara water have some traces of arsenic, and are of quite a different character from the ordinary mineral waters of the district.

If we take the next division, or the district between Pozzuoli and Naples, we shall find that this region continues to abound in mineral waters. It must have been thinking of them, rather than of the not very important waters of Naples proper, that Strabo said, the waters of Naples were in no way inferior to those of Baiæ, except in number. The baths he considered to be equally well furnished.

At the present day, there are no fewer than five bath establishments along the road from Pozzuoli to Bagnoli. Leaving Pozzuoli for Naples, we first pass, on the left hand, the baths of Subveni Homini, "Succour man," called also Del Ponte. They contain less carbonate of soda and more of the sulphates, than the waters of Bagnoli. The temperature of the waters varies, but may be considered to be about 104°. In modern times, the waters are chiefly used in nervous and paralytic affections and in dyspepsia. If the lines of Alcadinus are true, every gouty man ought to make pilgrimage hither.

"Hic etiam deponit onus longæva podagra,
Hic datur articulis induciata quies."

Close by, and on the sea-shore, but now entirely forgotten, were the sands of St Anastasia, used for purposes of arenation, i.e., of burying the body in heated sand. If a hole was scooped in the sand, hot water flowed in. As Alcadinus expressed it—

"Res miranda quidem, quicunque cavabit arenam,
In medio fossæ fervida manat aqua."

There is no trace of this old station for arenation, a process which we shall hear of again. The two next bathing establishments along the road, doubtless represent the old Bagni de Calatura and Petræ, the first in old days a cure for cough and phthisis, the latter for stone. The use of the waters on this coast must be reviving, if one may judge from the number of these establishments, which were all newly painted, and looked attractive, though they were closed at the time of my visit.

We come next to the very considerable bathing establishment of Bagnoli—corresponding to the old Balneum Plagæ, so called as being close to the sea-shore, for the two old baths not very far off, between it and the roots of the mountain of Pausilippo, the Balneum Juncaræ and the Fons Cryptæ, appear to be no longer used. The waters of Bagnoli, Alcadinus tells us, were the most popular of all with the people of Naples, and none are at the present day situated so conveniently for them. There are three springs—the temperature of the warmest about 105°. Their constituents seem to be nearly the same as those of the temple of Serapis, than which they are probably a little stronger. There is a very good establishment, with thirty bathing cabinets painted in Pompeiian fashion, and with two piscinæ, one for sitting, the other for swimming in. There are also chambers where pulverized water is inhaled. The waters are chiefly employed for bathing; but as they contain carbonic acid and soda and common salt, they may also be drunk, and are useful in vesical and uterine as well as in dyspeptic affections. The cure may be carried on at any season of the year. The views across the bay to Baiæ and Miseno and to the neighbouring island of Nisida are charming.

On the way from Bagnoli to Naples it is usual to turn off by a road on the left, and visit the stufas of San Germano and the Grotto del Cane. One comes first on the excavations in the tuffa known as the Sudatorium Agnianum or of San Germano. The latter name introduces us to a curious early legend as related by Gregory the Great in one of his dialogues, about the end of the sixth century. He says, he was told, when a young man, that Germanus, bishop of Capua, was sent to these thermæ by his physicians. It appears that Germanus, to his great astonishment and dismay, recognised among the servants of the establishment Paschasius, a Roman deacon, who had been dead for some years. On addressing him, Paschasius told him that he had never acknowledged Pope Symmachus, but having voted for his rival Laurentius, he still clung to him in affection, although overpowered by the general voice in favour of Symmachus; that for this venial offence (although he had been a man of such sanctity that a demoniac had been cured by merely touching his dalmatic, placed on his bier), he had been sent to purgatory, and was serving his time in the labyrinths of Agniano. He begged for the prayers of Bishop Germanus, and not in vain—for when the bishop came back next season, Paschasius was there no longer. These remarkable events are thus summarized by our poet:—

> " Illâ in Germanus Capuæ caput æde repertum,
> Ad Sacra Pascasi pascua te retulit."

These stufe must have been in a very different condition then, from their present one. Although they can, like the stufe of Tritoli, afford any amount of sudation—the heated air reaching the temperature of 147° and more—the arrangements are rough, and only suited for the poorer classes of patients. The Romans seem to have had at this spot thermæ on a large scale. They were said to have been built by Lucullus, and their extensive remains were to be seen in Hamilton's day. I have nothing new to say of these stufe, or of the neighbouring Grotto del Cane. It has been visited by every traveller, and experimented on by every philosopher who has got as far as Naples. I did not witness the usual experiment of first asphyxiating a dog by putting him into a hole, the bottom of which is filled with carbonic acid, and then restoring him. Natural escapes of gas are not uncommon, and here, as at other places, I was satisfied with perceiving the feeling of warmth which the gas communicated to my legs, and the strong, pricking sensation in the mouth and nostrils on dipping my head for a second. As regards the cruelty of the practice, the dog usually operated on trotted with us to the grotto quite readily, and seemed rather disappointed than otherwise, that we made no use of his services. A few yards from the Grotto del Cane is another escape of carbonic acid, accompanied by a certain amount of ammonia. It can scarcely be called an issue of ammoniacal gas, as t is described by some. I may observe that in the stufe close by, as well as in some of the baths in Ischia, there was a distinct deposit on the walls of sal ammoniac. The stufe and the grotto are close to the shores of what once was the Lake Agnano, which used to be considered bottomless, and whose waters, apparently containing some bubbles of gas, were supposed to have special virtue in restoring the asphyxiated dogs. It has been recently drained, and the land reclaimed is being gradually brought under cultivation—a measure which is expected to remove the character of the neighbourhood for insalubrity. As there has been much difference about the origin of the meaning of the word Agnano, or Aniano, I shall say a little about it.

The Lake Agnano was not described by the ancients at all. Gregory the Great first mentioned the Thermæ Angulanæ. This has unfortunately been commonly read Angulares, which has been converted into Angularia, and has led to the derivation of the name from the number of snakes or small eels supposed to be present in the waters, or from something about the configuration of the lake being angular. However, it is a very easy transition from the old name Angulanus to Agnanus. I cannot say what the word Angulanus means; there were a set of people in the centre of Italy called Angulani; and what is very curious, at another Agnano, close to the acidulous springs of Asciano near Pisa, there are emanations strong enough to kill small animals, and with a little lake beside it.

Those who are curious, may drive past the western side of the dried lake, cross the small stream running from the hot springs of Pisciarelli, and ascend the opposite hill of Astroni. Here is the strange and picturesque sight of an extinct crater, now beautifully wooded, and walled in as a game preserve, all round a circumference of about six miles. In the bottom of the crater there are some small wooded elevations, and at their base two small lakes, near the first of which there were a thermal spring and a thermal establishment, which fell into decay nearly two hundred years ago, and of which no trace can now be found.

Before quitting this side of the Bay of Naples, I may mention that some of our ships of war were, a few years ago, anchored for several months in the Bay of Pozzuoli, and that the crews remained healthy, although some of the officers suffered from fever, after being allowed to go on shore and shoot in the neighbourhood of Baiæ.

(*To be continued.*)

ARTICLE IX.—*Report of and Observations upon a Case of Puerperal Tetanus.* By ANGUS MACDONALD, F.R.S.E., Lecturer on Midwifery and Diseases of Women and Children.

(*Read before the Obstetrical Society of Edinburgh, 14th April.*)

ABOUT 10 A.M. on the morning of the 9th of March 1875, I was hurriedly summoned by Mr Bentley, one of my pupils at the New Town Dispensary, to see a patient to whom he had been that morning called, and who, he stated in the note, seemed to suffer from puerperal convulsions. The patient's name was Mrs Louttet, her age 24, and her residence Ironside Close, 85 Abbeyhill.

She had been married for three years, and had had two children. The eldest, a healthy girl, was born about one year after marriage, and is still alive and well. This child, however, was suckled by the patient during the whole period of her second pregnancy, and even up to the commencement of her labour.

The second confinement took place on the 26th day of February 1875, at 3.50 A.M., when she was delivered by Mr Bentley of a male child, after an easy labour of a few hours' duration. All the stages were natural and normal. The placenta was removed about fifteen minutes after the birth of the child, with little or no effort on the part of the attendant.

Judging from the appearance of the second child, as also from the expectations of the mother in regard to the period of her confinement, the labour would appear to have supervened about three weeks or so before the completion of the full term of utero-gestation. Except some slight appearances of prematurity, however, such as deep colour of skin, slightly defectively-developed nails, and rather more than ordinary lanugo on the shoulders, the child was healthy and well formed.

The patient had continued to progress favourably, exposing herself to no special risks, from getting up too early or otherwise, and had looked after the proper action of her bowels by taking twice castor-oil, which had acted well on both occasions, until about 2 A.M. on the morning of the 9th of March, when she was noticed to be sitting up in bed, bent forwards. It is to be noticed that on the previous day she had been in her usual health, going about her duties, and making no complaints. When she sat up at this time she complained of no pain, and in fact never spoke, but appeared very restless, rubbing her legs and clutching at her hair.

It is impossible to obtain any exact information from the attendants as regards the first onset of the tetanic spasms, but it is clear that there was heavy breathing from about 6.30 A.M.

On Mr Bentley's arrival at 9 A.M., the patient was completely unconscious, and there were present strong tetanic spasms, similar to those described below, only more violent in character. The perspiration stood out in beads on her head and face.

The patient's condition on my arrival was as follows:—

Face pale; general appearance anæmic; skin soft and dry, but feels as to temperature natural; pupils somewhat contracted, and almost completely insensible to light; conjunctivæ insensible to touch; eyes glazed-looking. Patient completely unconscious both during and between the attacks of spasm. Urine expelled involuntarily. Spasms of a truly tetanic character recur at intérvals of about a minute. During these seizures the heels and the occiput are the only parts of the patient's body which touch the bed, the contractions being thus mainly of an opisthotonic character.

During each seizure the muscles of the back are seen to be strongly contracted. The upper extremities are strongly flexed, the fingers being bent forcibly towards the palm, and the thumb turned inwards, and the forearm powerfully flexed over the upper arm. If any predominance of contraction on one side of the body over the other exists, it seems to be on the right; but it is slight, if it exists at all, and seems rather to be an appearance due to the patient lying slightly over in bed towards the left side.

The slightest irritation applied to the skin, such as the touch of a cold hand, is sufficient to throw the patient into complete opisthotonos, which attacks are also induced by every effort at examining the patient's condition.

The muscles of the neck are powerfully contracted during the attacks, more particularly the sterno-cleido-mastoid; but there is no risus sardonicus, and it is only during the more severe attacks that trismus occurs. Every 1½ minute or so, an attack occurs, and every fourth or fifth attack is much more severe than those intermediate. There is no congestion of the face during the spasms. Though the opisthotonos can be induced at any time by application of external stimuli, the attacks recur at regular intervals of about 1½ minute spontaneously.

Pulse 74, weak, soft, compressible, and somewhat irregular. Respirations, during intervals between the fits, shallow and quick, being 26 per minute, but during the attacks they become stertorous. Temperature per vaginam, 101°.

Attempts to get the patient to swallow induce violent fits, but prove wholly abortive.

On examination per vaginam and per hypogastrium, uterus and vagina seemed perfectly normal; no ulcer or sore to be found. The cervix uteri felt soft, still large and patulous, and from its external orifice some mixed sanguineous discharge soiled the examining finger. The uterus, both as to size and position, seemed perfectly normal, being slightly anteverted, as Credé[1] has shown it should normally be about this period. No unnatural fulness or resistance in the pelvis to be recognised. No scar or wound visible on extremities or trunk, except some eczema pediculare of scalp.

A catheter was now passed, and about a tablespoonful of water extracted from the bladder.

This, on cooling, exhibits a profuse deposit of urates, is of acid reaction, but on heating with nitric acid does not give the slightest trace of albumen.

The following treatment was then ordered:—

℞ Hydratis chloralis, . ʒiii.
Aquæ, . . . ℥iii. Solve.

Sig. A tablespoonful to be mixed with a little warm water and injected into the rectum.

At 11.30 A.M.—The spasms now recur every minute, and last about two seconds, and are accompanied with a stertorous breath or two. First dose of chloral injected, and apparently retained. Some slight amount of fæces found in rectum. The act of injection set up a fit of unusual severity, accompanied with grinding of the teeth.

[1] Archiv für Gynäkologie, band i. s. 96.

12.30 P.M.—Pulse 76 per minute; respirations, 24½. Character of pulse and respirations same as before. Spasms same in character, but recurring fully more frequently.

Another half-drachm of chloral injected and retained.

1.10 P.M.—Eyes have lost their glazed appearance; pupils rather more dilated; tongue clean and moist; lips, however, are dry.

1.20 P.M.—Breathing more stertorous and laboured. Inspiration accompanied with a loud gurgling sound, air entering the lungs very imperfectly. Occasionally a deep sighing inspiration occurs, without stertor or difficulty. Spasms occur less frequently.

Pupils now well dilated, right more so than the left. Pulse 88, slightly irregular, but not so soft as before, being now rather bounding in character.

1.45 P.M.—No spasms have occurred for twenty-five minutes. Inspiration now accompanied with a crowing sound—stridulous.

2.10 P.M.—Breathing not so stertorous; face flushed in patches; no fits.

2.12 P.M.—Face very livid; respiration ceased; pulse 120, very soft and small; body and extremities moist and warm; pupils dilated as before.

2.13 P.M.—Pulse much slower and regular, 72 per minute.

2.15 P.M.—Pulse at wrist, as also heart's impulse, imperceptible. Patient dead.

For these exceedingly minute and accurately detailed notes I have to express my obligations to Mr Bentley and Dr John Playfair, both of whom never left the patient's bedside from the time of our first arrival till she died.

After some difficulty, we succeeded in persuading the friends to allow of a post-mortem examination. This was performed on the 11th by Dr John Wyllie, with the following results:—

Post-mortem Examination, Forty-six Hours after Death.

Brain.—On opening the skull, about two ounces of serous fluid escaped from the lateral ventricles. Cerebral convolutions were flattened, and some venous congestion of the cerebral surface present. Both lateral ventricles were filled with dark blood-clots, which passed forwards into the anterior cornua, and backwards into the posterior cornua. The amount of clot seemed to be about half an ounce in each ventricle, and there was some laceration of the ventricular walls. In each corpus striatum there were a vast number of small extravasations, varying in size from that of a millet-seed to that of a pea. This condition was found to extend throughout the entire tissue of both corpora striata. The third ventricle also was filled with blood-clot, which again was found to extend along the iter to the fourth ventricle, and hence to the inferior sulcus between the two lateral lobes of the cerebellum.

As a result of the hæmorrhages into their tissue, both corpora striata were torn up and softened.

The venæ Galenæ and the straight sinus were found occupied by a large, dense, firm, and adherent thrombus. This condition extended also into the left lateral sinus, but in its cavity the clot was not so firm. The blood in the right lateral sinus was fluid.

Heart.—Right ventricle and pulmonary artery contracted. Some partially decolorized and rather tough blood-clot in them; but this clot was not adherent. Left ventricle empty and healthy. *Liver* healthy. *Lungs* not examined.

Uterus.—Cervix somewhat congested. Mucous membrane throughout moistened with a slightly grumous or sero-sanguineous fluid. The placental area still could be made out from its distinctly darker appearance than that of the rest of the mucous aspect. The muscular and peritoneal coats seemed perfectly healthy. The mucous membrane seemed abnormally thin, but otherwise healthy.

A rather noisy assemblage in the adjoining room now became so specially demonstrative, that we had to desist from further examination of the body.

We managed, however, to carry off the uterus for further examination.

There are so many points of special interest in regard to this case, that I have deemed it of sufficient importance to bring it before the Society, trusting that its relation and discussion may prove interesting and instructive.

In the outset, I wish to state that, though I have called this case one of puerperal tetanus, I do not regard it to be a case of ordinary traumatic tetanus, but really one of tetaniform convulsions, arising from apoplectiform congestions and extravasations. But in naming it as I have done, I have acted not inadvertently, but advisedly, as I have been led to believe that the disease known as puerperal tetanus may be better explained in this manner than by regarding it as essentially the same as traumatic tetanus.

Into the question as between the theory which maintains a purely nervous origin for tetanus, and that which refers its causation to inflammatory or at least congestive changes, I do not at present enter at length, but mean to refer to it further on in the paper. I therefore pass on at once to the consideration of the more salient features of this peculiar case.

While, in some particulars, the case presented phenomena which suggested a close relation between it and ordinary eclampsia, there were a large number of uncommon symptoms, which struck me as differentiating it very sharply from an ordinary case of eclampsia.

I also think that the President, whom I asked to see the case with me, will bear me out in this statement of opinion. The chief distinguishing points were—

1st. The spasms were distinctly *tonic* and not clonic, or mixedly tonic and clonic, as we find in cases of eclampsia.

2d. The pupils were somewhat *contracted* and *fixed*, instead of *dilated and fixed*, as is the ordinary condition in deep eclampsia.

3d. The face continued pale during the seizures, instead of becoming congested, as in eclampsia.

4th. The contractions were very markedly more powerful in the posterior aspect of the body, so as to establish a condition of more or less continuous and complete opisthotonos.

5th. The rate of recurrence of the spasms was much more rapid than in any case of eclampsia that I have witnessed, and my experience refers to a large number of cases.

6th. The spasms were capable of being excited and intensified by the slightest external stimulus or irritation, such as the touch of the cold hand, or examining any part of the body by tactile manipulation, which is not the case in eclampsia.

7th. There was no albuminuria present, which we generally, though not invariably, find in cases of puerperal eclampsia.

On the other hand, the suddenness of the first onset of the attack, the stertorous breathing during the individual seizures, the complete unconsciousness of the patient, and her general aspect, with the above-mentioned exceptions, forcibly reminded one of eclampsia. Still, however, the great preponderance of symptoms tend to prove the existence of a tetanic rather than an eclampsic condition.

This being so, we are naturally led to attempt to solve the question, What were the predisposing, and what the proximate, cause of these alarming symptoms and conditions, which collectively formed the phenomena of the case, and resulting from which the death of the patient so speedily followed?

I hesitate not to state, that I firmly believe that that peculiar condition of the blood (septic?), allowed on all hands to exist during the puerperal period, must have been the main predisposing element. The patient, it must be borne in mind, had, in addition to this, been weakened by prolonged lactation, and was consequently in feeble health at the time when her confinement supervened.

A *specially* vitiated condition of blood in this particular case is further rendered exceedingly probable, by the fact that the mucous membrane of the uterus was imperfectly developed. The ordinary attempts at reproduction of it had manifestly been unduly arrested, as was proved by carefully conducted microscopical examination, whilst the placental area was of an abnormally dark hue. These are precisely the conditions under which we find the formation of venous thrombi; and, as a matter of fact, there had been slowly forming for some days, in consequence of them, thrombus in the venæ Galenæ, in the straight sinus, and in the left lateral sinus.

Into the venæ Galenæ open the venæ corporis striati, the venæ choroideæ, as also some cerebellar veins, whilst, again, the venæ

Galenæ pour their contents into the straight sinus, there commingling with the current of blood which passes through the inferior longitudinal sinus, and with the contents of the inferior median and of the superior cerebellar veins.

Those portions of the brain draining into the straight sinus, whether directly or indirectly, must of necessity have been thrown into a condition of intense venous congestion so soon as the impermeability of the straight sinus was established.

This congestion, it appears to me, was the proximate cause of the tetanic convulsions. Such a case as this, therefore, I hold, in its essential features, differs little, if at all, from a case of phlegmasia dolens, or of pulmonary thrombus, except in this particular, that here the thrombus occurred in a portion of the body closely related to the centres of organic life, and the delicacy of whose anatomical arrangements permitted less free deviation from the ordinary boundary of healthy action without leading to consequences immediately disastrous.

Consequently, whilst thrombus of the femoral tends to the establishment of the comparatively innocuous swelled leg, and thrombus of the lung, or of any other organ, leads to results more or less injurious according to the importance and amount of the tissue concerned, thrombus of the venæ Galenæ and straight sinus leads to convulsions of a tetanic character, accompanied, in this case, by destruction of the tissue of the corpora striata, apoplexy into the cerebral ventricles, and followed by speedy death.

Let us now consider in what manner this congestion led to the tetanus.

The investigations of experimental physiologists, and more particularly and lately those of Ferrier and M'Kendrick, tend to establish the belief that the corpora striata and the corpora quadrigemina are the great motor centres for the whole body. These views are also supported by the experiments of Tenner and Kussmaul, if I rightly interpret the result of their experiments. For it will be noticed that, when they made their sections very deep, so as to interfere with the structure of the corpora striata and the corpora quadrigemina, convulsions of a tetaniform nature resulted from the irritation, whilst the conditions capable of inducing epileptic convulsions on compressing the carotids were found to be more or less entirely lost, *i.e.*, as I read it, the motor centres had been first irritated, and then more or less entirely destroyed by the previous sections.[1]

Whilst irritation of these great motor centres, as a whole, induces general tetanic spasms, it has been further shown by Ferrier that irritation of the corpora quadrigemina is followed by opisthotonos. We have, therefore, in the condition of venous congestion of these great motor centres, a sufficient irritative stimulus to account not only for the opisthotonos, but also for the existence of general convulsions of a tetanic nature. Why, therefore, it may be asked, did the opisthotonos so decidedly preponderate?

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[1] Kussmaul and Tenner on Convulsions after Hæmorrhage, experiments 7, 8, 9, and 10, pp. 77, 78.

In the first place, I would take leave to notice that the patient was not seen by Mr Bentley until after she had suffered from the attacks for several hours—in fact, for five or six hours; and we have, in consequence, no means of determining whether the seizures were not of a more general character at first, and even whether they were not eclampsic.

In the second place, after the attack had lasted for some hours, and the extravasations into the ventricles and into the tissue of the corpora striata had advanced so far as to lead to pressure upon and destruction of the tissue of these ganglia, their functional activity must necessarily have failed, and, instead of exalted motor activity in the muscles regulated by them, arrest of such action must have resulted.

Up to the last, however, the corpora quadrigemina were in a different condition. Though the great mass of blood supplied to them drains into the straight sinus, and though, consequently, the same condition which congested and broke up the corpora striata led to irritative congestion of the former ganglia, yet, whether, owing to the vessels in relation to them being of stronger tension, or for some other reason, the congestion had not in their case led to extravasation, and consequently we had exaltation of their special function in the form of opisthotonos.

There is some difficulty also in accounting for the sudden onset of the seizures, as, from the amount and the firmness of the venous thrombus, we cannot doubt but it must have existed for some days previously to the commencement of the fits. But I think that this is rationally and satisfactorily accounted for by the supposition that only immediately before the attack did the venous sinuses become absolutely closed, having previously admitted an amount of blood to pass, sufficient to prevent the occurrence of that degree of congestion capable of determining in convulsions.

And now to consider the general question of puerperal tetanus, and more especially its relation to septicæmia. I regard this case as one specially valuable in its bearings upon this point.

That partial tetanus may result from congestion and inflammatory injuries of the cerebral ganglia, and also the upper part of spinal cord, as well as from other organic lesions of these parts, is manifest; and, further, such a result is allowed for in the cases referred to by Tenner and Kussmaul, p. 96–98, and by Curling, pp. 52 *et seq.*, as also in his Synopsis, at page 122. The theory that tetanus is *invariably* consequent upon some inflammatory lesion of the spinal cord or of its investing membranes, has been always maintained by a certain class of surgical pathologists, and notably by Baron Larrey, whose experience of such cases was certainly very great.

Now, though Curling maintains, and I think rightly so, that lesions of an inflammatory kind cannot be made out in ordinary cases of traumatic tetanus, I conceive that he errs in dwelling too exclusively upon the idea of actual inflammation being necessary to be proved in such cases in order to show that a cause external to the nervous tissue itself produced the disease. He is, in my opinion, thereby led to put too much stress upon a peculiar irritable condition of the nervous system as the element essential to tetanus. After all, it must be remembered that such a condition is a pure hypothesis. If only we could account for tetanus on the ground of disturbed vascular supply to the motor centres within the cranium, we should have something much more tangible to deal with than any imaginary irritability of these centres.

I do not pretend to experience warranting me to pass an opinion of value in regard to ordinary traumatic tetanus; but from all I can find out respecting it, I am inclined to believe that only a small proportion of the cases can, in the present state of our knowledge of the pathology of the disease, be referred to vascular changes. The great majority of them, indeed, would seem to me to warrant us in assuming that there exists in the nervous system itself, however this may have been induced—a special condition, which is the main occasion of the spasmodic seizures.

On the other hand, my study of the disease called puerperal tetanus leads me to quite an opposite conclusion regarding it. I do not think it is the same in nature as ordinary traumatic tetanus, but that it is more allied to eclampsia; and a careful study of the recorded cases of it more and more convinces me that most, if not all, of them can be referred to disturbance of vascular supply through venous stasis in the great motor centres, or to some such cause acting similarly to the thrombus which occurred in this case. In the case before us we have no evidence of any inflammatory action at work in the motor centres; but we have the most manifest proof of intense venous stasis, even leading to rupture of the vessels in the corpora striata and disruption of their structure.

Had, in our case, the vessels remained persistent, and had the extravasations of blood which led to destruction of the tissue of the corpora striata and the formation of apoplectic extravasations, with consequent pressure upon the cerebral lobes, and unconsciousness, not taken place, and thus speedy death been averted, it seems to me that, beyond reasonable doubt, we should have had a case of tetanus which would have resembled much more closely the ordinary traumatic form of the disease. In that case, we should have had life prolonged for several days, along with complete consciousness; for we have every reason to believe that the corpora striata and the corpora quadrigemina, the parts concerned in the consecutive congestion, are entirely beyond the centres of conscious existence.

Let us suppose, then, such a case thus progressing until the patient died, not of apoplexy, as in the present instance, but of exhaustion, and that no post-mortem had been, as in our case, granted, we should have been fairly entitled to have recorded it as an instance of true puerperal tetanus, instead of a case of apoplexy with tetaniform convulsions.

On looking over the cases in Sir James Y. Simpson's Selected Obstetrical Works—and he has made the best, nay, almost the only extant collection of such cases—I have been more and more struck with the aptness with which the supposition that venous congestion of the great motor tracts in the cranial portion of the spinal cord had been set up by some embolic obstruction, such as occurred in the present case, fits in as an appropriate explanation.

It is much to be regretted that the records of his cases, and particularly in regard to post-mortem appearances, are sadly deficient. But a careful scrutiny of the cases fairly entitles us to hold that exactly the same class of conditions are present which ordinarily are apt to lead to blood-poisoning and the formation of embolism or of thrombus. I would here take leave to remark, that Heiberg throws in the authority of his name and of his experience on the side of the occurrence of embolism or thrombus being rather secondary to blood-poisoning than a primary symptom, and points out that we have erred in looking upon the embolism in cases of septicæmia as a cause rather than as a result of the poisoned blood.[1] But to return to our subject.

The cases recorded by Sir James Y. Simpson are in a great measure connected with abortion, where, owing to hæmorrhage, plugging of the vagina had been required, or in which portions of the membranes had been retained, the patients being weak, and weakened with hæmorrhage; or they are stated to arise in connexion with premature births, or delivery at full time during the puerperal period, coincidently with the onset of feverish symptoms, and usually accompanied with suppression of the lochia. In this class of cases, also, it is noticeable that hæmorrhage or other weakening cause is frequently stated to have been present. Cold is usually referred to in the brief records as being the exciting cause of the symptoms. This, however, seems to me to indicate rather the commencing rigor of the inflammatory mischief; and it appears to me that, at most, the cold may have given origin to an endometritis, a paraperimetritis, or some such ailment, from which the tetanic conditions ultimately, though indirectly, arose.

It is further worthy of notice, that the cases usually come on about that period when a phlegmasia dolens is most commonly found to become established, namely, about the tenth or fourteenth day after confinement.

In Dr William Craig's case, reported to this Society on the 23d of February 1870, it is distinctly noted that there was no evidence of metritis nor of peritonitis. But then it is to me exceedingly interesting, as tending to establish the pathological view I have been advancing, to remark, that Dr Craig states that the patient had on previous occasions suffered from extensive varix of the right leg, which burst some days previously to her confinement; and, besides this, that the patient had suffered from rather severe hæmorrhage before the midwife in attendance sent for Dr Craig to remove the placenta. Also, it is stated, that between then and the ninth day, when the patient began to suffer from symptoms of tetanus, there had existed a very suspiciously quick pulse.

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[1] Hjalmar Heiberg, Die Puerperalen und Pyämischen Processe, s. 3. Leipsig, 1873.

I am not aware what may have been the experience of the Fellows of this Society, but mine has been such as to lead me to keep a sharp lookout in case phlegmasia dolens should supervene, whenever, without manifest cause of an uterine inflammatory nature, I have a quick pulse persisting for some time after labour, say eight to ten days, in a woman of a weak constitution, especially if she has been further exhausted by post-partum hæmorrhage.

I may state that Karl Schroeder expresses himself as of opinion that puerperal tetanus must be referred to some septicæmic influences.[1] Whether these causes act, as I have been led from the study of this interesting case to indicate, is, I frankly grant, as yet extremely uncertain. All I mean to say in reference to this matter is, that the above commentary is a hurriedly-prepared and confessedly imperfect attempt to account for the occurrence of the disease called puerperal tetanus, which I feel certain is a disease differing *in toto cœlo* from ordinary traumatic tetanus. The more I read and ponder the scanty literature we possess in regard to this obscure subject, the more I feel convinced that no explanation similar to what is commonly given for the occurrence of traumatic tetanus will suit puerperal tetanus; and, on the whole, the predicating of a venous congestion of the cranial centres of motion by the formation of phlebitic thrombi, seems to be, viewing all the facts of the cases, the most likely explanation of the phenomena observed.

Part Second.

REVIEWS.

Clinique Médical de l'Hôtel-Dieu de Rouen. Par E. Leudet, Directeur de l'Ecole de Médecine de Rouen, etc., etc. Paris: J. B. Baillière et Fils: 1874. 8vo, pp. 606.

This is not a work on clinical medicine as this term is generally understood; it is a series of medical essays, in which a large amount of clinical observation is employed to illustrate the pathology and treatment of certain more or less rare forms of disease. The title, therefore, is misleading, and the work is more valuable than we might suppose. The first essay is one of some importance upon suppurative pylephlebitis consecutive to diseases of the liver and bile-ducts, the most characteristic symptoms of which were jaundice, accompanied by repeated rigors. In the second essay, upon inflammatory affections of the liver resulting from the abuse of alcoholic drinks, the author reckons amongst these an acute atrophy of the liver, with typhoid symptoms; acute jaundice, with augmented volume of the liver; a more permanent enlargement of the liver, of slower growth, accompanied by fatty and waxy degeneration—an affection of the liver occasionally observed in this country, in which the source of the waxy degeneration has hitherto proved a puzzle, from the point of view generally recognised here of its being a dealcalized fibrine, the result most usually of preceding profuse suppuration. Leudet seems to regard it simply as a form of retrograde metamorphosis of the tissues. Whether he be right or wrong, it is however a clinical fact of some importance to have waxy degeneration distinctly recognised as occasionally occurring as the result simply of the abuse of alcoholic drinks. He also recognises the frequency with which chronic hepatitis of this character commences in hypertrophy and ends in atrophy; but he abandons the use of the term cirrhosis, as only applicable to the advanced stages of chronic interstitial hepatitis, while enlargement of the liver depending on the same cause does not, according to him, always nor of necessity terminate in atrophy. Leudet's sixteenth essay, on the Influence of the Abuse of Alcoholic Drinks on Pulmonary Tuberculosis, is also of considerable importance, for he positively states, as the result of his experience, that the abuse of alcohol does not influence ("ne provoque pas") the development of pulmonary tuberculosis; that phthisis is rare among drunkards; that miliary tubercle is not more frequent among drunkards than among others, and that they do not succumb to its ravages more rapidly than others. Thus in so far confirming the results recorded by Dr Bowditch in the Fourth Annual Report of the Massachusetts Board of Health, where he states, that of 210 medical men who were asked—Question sixth, Is consumption prevented by the drunkenness of an individual? 21·9 per cent. declined to answer, 8·09 per cent. doubted, 53·8 per cent. answered in the negative, 12·86 per cent. answered "Yes," and 3 per cent. stated that drunkenness retarded the progress of consumption. In reference to these replies, Dr Bowditch says, "In the present state of public opinion in regard to the use of intoxicating drink, it requires some moral courage to say anything in favour of alcohol. To declare that it sometimes seems to save the drunkard from the consumption to which he is hereditarily predisposed, requires not only moral courage, but a sincere conviction of the truth of the assertion." Without pursuing the subject further now, we may commend the researches of Bowditch and Leudet to the consideration of those well-meaning men who are so one-sided as to see nothing but unmitigated evil in the use of alcohol. The remaining essays of M. Leudet are of varying importance, but they are all of considerable clinical interest; that upon the various circumstances which occasion a want of tolerance of tobacco-smoke possessing indeed quite a general interest; and we may also specially notice the production of polyuria as the result of chronic meningitis, a form of polyuria which has been little attended to in this country, though, when we reflect on the symptoms occasionally observed in connexion with cases slumped together as diabetes insipidus, we are convinced it is a very probable cause of not a few of them. We have much pleasure in recommending M. Leudet's essays to the consideration of the profession.

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[1] Lehrbuch der Geburtshülfe, s. 657. Dritte Auflage. 1872.

A Clinical Pocket-Book for the Use of the Students of the Manchester Royal Infirmary. London: Smith, Elder, and Co.: 1874. Pp. 29.

THERE is no name on the title-page of this little book, though, from the initials "W. R." appended to its preface, we imagine Dr Roberts may without injustice be credited with its paternity. If so, we would have expected better things from him. But it is no worse than other booklets of the kind; certainly not better. We are content to leave it to the use of the students of the Manchester Infirmary till they get a better, which we hope may be soon.

Manual of Pathological Anatomy. By C. HANDFIELD JONES, M.B., F.R.S., etc., and E. H. SIEVEKING, M.D., F.R.C.P., etc. London: J. and A. Churchill: 1875.

THE appearance of a second edition of Jones and Sieveking's "Manual of Pathological Anatomy," after a lapse of nearly twenty years, cannot be otherwise than gratifying to its authors. The fact of a second edition being called for shows that the work, originally much appreciated, is still in demand. As, however, many cardinal changes have been introduced into pathological anatomy during the past twenty years, a careful revision and extension of the work were necessary. This task has been intrusted to, and on the whole well performed by, Dr Payne, late Demonstrator of Morbid Anatomy at St Thomas's Hospital. Dr Payne, like many other editors, has culled much of his new material from standard authors, and failed to give the references. Indeed, he states in the preface that "*it has not been thought necessary as a rule to refer to special passages of these works.*" Now, this is a custom much to be deprecated. If a passage, or the substance of a passage, is worth extracting, common honesty renders the mention of the author's name imperative. The compiler, as every one knows, s not to be placed on the same platform with the original investigator.

The Manual, in its revised and enlarged form, has, we are happy to be able to state, been adopted as the text-book of pathology in he University of Edinburgh—a fact of some significance, considerng the careful manner in which this department of medicine is at present taught in that celebrated school. Concerning the text we have little to say. It is carefully and judiciously written, contains he latest information, and gives a fair summary of pathological anatomy as at present taught at home and on the Continent. With the illustrations we are not so well satisfied. Many of these are diagrammatic in character, and present a blurred appearance, ndicative of wear and tear. Wood engraving is not well adapted o the delineation of pathological appearances, as colour as well as form must be studied in such representations. When, however, wood engraving is employed, the work should be of a much higher class than is found in the manual before us. Effects in wood engraving are only obtained by finish, and a scratchy woodcut is as misleading as regards pathological appearances as a badly-drawn igure is misleading as regards form. We have no wish to be hypercritical, but it becomes a question whether illustration should be introduced when it cannot be done at a moderate cost, and in such a manner as to make it really intelligible. Fig. 107, page 481, *e.g.*, does not convey to our minds the idea of a splenified lung affected with typhoid pneumonia; still less does Fig. 111, page 496, convey an idea of miliary tubercle of the lung. Both of these figures consist of plain ruled lines, with a few unintelligible blotches and rude attempts at shadow. Not the slightest trace of structure is to be found in either. Similar remarks are to be made of Fig. 139, page 616, Fig. 141, page 623, Fig. 143, page 636, which figures represent different diseased conditions of the liver. Fig. 181, page 773, representing incipient cyst formation in the ovary, is as hard and sketchy as it can well be. In fact, one would imagine the ovary was crowded with pebbles instead of cysts containing fluids. Fig. 190, page 834, wears an air of antiquity, and is sadly wanting in definition. Numerous other examples might be cited, but a sufficient number have been adduced to ustify the foregoing remarks.

If the present manual reaches a third edition, which is exceedingly probable, we hope the authors will endeavour to make the illustrations worthy of the text. Taken as a whole, the work, we venture to predict, will be well received by the profession. To those who have not the time or the learning requisite for consulting the great French and German works on pathological anatomy, the present volume will form a real acquisition.

Lectures on Pathological Anatomy. By SAMUEL WILKS, M.D., F.R.S.; and WALTER MOXON, M.D., F.R.C.P. London: J. and A. Churchill, New Burlington Street: 1875.

DR WILKS, as we learn from the preface to the first edition of his Lectures, has had a very special training as a pathologist, having made a daily study of the dissection of the dead for fifteen years, having recorded between 2000 and 3000 post-mortem examinations, and having superintended the splendid pathological museum of Guy's Hospital.

Dr Wilks, it will be observed, has studied pathology under unusually favourable circumstances. He has, moreover, a genius for original observation and research; and this has led him to examine critically, and for himself, the various topics discussed in his pages. He is very explicit on the latter point, for he informs us, "I have carefully abstained from making any statement unverified by my own observations and experience."

A course of lectures prepared under such favourable auspices could scarcely fail to be a success, and a success the first edition of Dr Wilks's Pathological Lectures undoubtedly proved. It will not be too much to say, that their appearance marked an era in the pathological anatomy of Great Britain.

The same conscientious, intelligent zeal which characterized the first edition of Dr Wilks's Lectures characterizes the second. The preparation of the second edition has been entrusted to Dr Moxon, late Demonstrator of Anatomy at Guy's Hospital, and to this gentleman the thanks of pathologists and the profession are due. Dr Moxon has followed Dr Wilks as to originality of treatment, his object being, "to preserve the general character and aim of the work."

As, however, the altered aspect of pathology in the present day rendered a revision, rearrangement, and extension of the Lectures necessary, Dr Moxon has been obliged to exercise the same industry in preparing the second edition which Dr Wilks exercised in preparing the first. "New material," Dr Moxon observes, "was required, in the furnishing of which observations of my own were necessary, in order to maintain the original character of the Lectures, and this required time and labour, whose results are embodied in the new portion of the work." Dr Moxon has refrained from illustrating the descriptions with figures, being of opinion, that coloured drawings were necessary, and that these would have made the work too expensive. In this we think Dr Moxon has acted judiciously, for it must be evident to all that a good description minus an illustration is preferable to a good description plus a bad illustration.

The volume of Pathological Lectures, in its extended form, has much to recommend it. It is an original work of great merit, and, as such, will take a high place in the literature of medicine. It is written in an easy style, and one feels, when reading it, that its authors have spared neither time nor pains in making it a realistic and typical work, each page disclosing either original research or original reflection. In this respect it differs from, and must take precedence of, every form of collated work however well written. No mere compilation bears the impress of independent thought, and the volume before us most indubitably does. We heartily recommend the Lectures to all lovers of pathology, and to all members of the profession who desire a sound knowledge of disease and post-mortem phenomena.

The Pathological Significance of Nematode Hœmatozoa. By T. R. Lewis, M.B., Staff Surgeon, H.M. British Forces (on Special Duty), attached to the Sanitary Commissioners with the Government of India. Calcutta: 1874.

This is a valuable contribution to pathology, and the conclusions arrived at are deduced from a series of most careful clinical and post-mortem observations. The nematode in the human subject, he finds, differs from that found in dogs—the one possessing a fine hyaline sheath, which is absent in the other; and the results of his studies lead to the opinion that chyluria and the elephantoid state so common in tropical countries, are dependent on, and associated with, the presence of a microscopic hæmatozoon, and that the symptoms presented in those diseases are due to mechanical interruption to the flow of the nutritive fluid in the capillaries and lymphatics. This belief, however, requires confirmation by more extensive observation, and we hope soon to possess further information on the subject through Mr Lewis's scientific researches.

A Practical Treatise on the Diseases of the Eye. By Haynes Walton, F.R.C.S. Third Edition. London: J. and A. Churchill: 1875. Pp. 1188.

We welcome a new edition of Mr Walton's treatise. Fourteen years have elapsed since the publication of the previous edition, and when we consider the wide strides in advance that ophthalmology has recently made, we are not surprised to find the widely-printed volume of 680 pages which constituted the second edition transformed into a closely-printed bulky tome of nearly 1200 pages. We may venture the remark, that it would conduce to the comfort of the reader were the thick, unwieldy volume divided into two.

The general arrangement of the work has been considerably altered in this edition, and several new chapters have been added, including one on the Anatomy of the Eyeball (written by Mr Norton), and one on the Anomalies of Accommodation and Refraction. We also observe that the chapter on the Ophthalmoscope is from the able and indefatigable pen of Dr Clifford Allbutt. The material which has been added was necessary to render this a complete treatise on diseases of the eye, which position Mr Walton did not claim for the previous editions. Considering the difficulties that must beset an author in active surgical practice who desires to write a complete and thorough book of reference, embracing all the most recent scientific investigations, and noticing the newest modes of treatment, on such an extensive and intricate subject as ophthalmic surgery, we think Mr Walton deserving of much credit for having produced a work which will take a place in the standard literature of the subject. From a general glance through the book, and a more careful perusal of some of the chapters on subjects that have been recently enlightened by scientific observation and research, we can speak most favourably of the clearness and precision with which the topics generally are treated, and with which the author's views and opinions are expressed. Some of the chapters struck us as particularly good, such, for example, as the introductory chapter on "Geometrical Optics," the chapter on the action of the muscles of the eye, and those on diseases of the orbit; while, on the other hand, some chapters fell short of our expectations.

It is our province to view with critical eye all parts of a work, and to point out defects where we find them; we therefore would direct Mr Walton's attention to a few inaccuracies and imperfections, in our estimation, which we would be glad to see rectified in the next edition.

At page 33, while treating of intraocular cysticercus, he thus refers to a case of extraction of the parasite from the vitreous humour, related by Von Graefe:—"He first performed iridectomy. At a subsequent period, he extracted the lens, which was clear and transparent; and finally, he succeeded in removing the cysticercus, the patient recovering from the three operations with a fair amount of vision. It is impossible to conjecture what really was the state of sight. The expression is too vague to be worth much." When we read this passage we of course imagined that the very indefinite account of the amount of vision regained by the patient was all that was given by Von Graefe, and knowing the great accuracy which usually distinguished the reports of cases by that renowned oculist, we in some amazement turned up the reference to the original paper given by Mr Walton, and there (Archiv für Ophthalmologie, band iv. abth. 2) found a most exact report of the patient's vision before and after the operation. Before the operation of extraction of the entozoon, the patient could only count fingers at one foot distance from the eye. Three weeks after the operation the visual power was the same; but two weeks later he could (with a convex glass of 4 inches focus, to make up for the natural lens which had been removed) count fingers at 4 feet distance; and with a lens (of 2½ inches focus) suitable for reading, decipher large print (Nos. 18 and 19 of Jaeger's types). Two months after the operation, the patient could count fingers at the end of the room, and read some words of smaller type (No. 14). Ophthalmoscopic examination revealed atrophy of the choroid, and a bulging backwards of the sclerotic, at a spot corresponding to the macula lutea, which affection Von Graefe naturally concluded had prevented a still greater improvement in vision. The case was, in fact, most carefully observed and reported. We therefore think the passage from Mr Walton's book which we have quoted requires modification.

The chapter on Strabismus is the one which we consider most to require revision. Mr Walton attempts to overturn the views at present held as correct regarding the dependence of convergent strabismus on hypermetropia. Mr Walton says he has in vain looked for squint with hypermetropia only. As is well known, in almost every case where one eye has squinted for a lengthened period, the vision in that eye is very much impaired, owing, it is supposed, to what is called "mental suppression" of the images formed on that retina. It is therefore very difficult to find cases in which, on correcting the hypermetropia, the vision is perfect; but a few such do occur, and have come under our own observation. Mr Walton may, of course, maintain that these are merely exceptions to the general rule, and cite on the other hand numerous cases of hypermetropia in which no strabismus has been produced. But as we find hypermetropia in an excessively large proportion of cases of convergent strabismus, we are inclined to believe the former stands to the latter in the relation of cause and effect. The want of the ordinary relationship in hypermetropia, between the amount of exertion of accommodative power and the degree of convergence of the optical axes, affords, to our minds, a much better explanation of the occurrence of strabismus than Mr Walton's plan of attributing it to defective sight independent of hypermetropia. We think that, if Mr Walton had fully considered the case he himself cites, at page 388, of a little patient of his who was hypermetropic, and always squinted when he looked at near objects, except when he looked through a sufficiently strong convex lens (to correct his hypermetropia), he would in that instance at least have admitted the force of hypermetropia in the production of strabismus. We cannot, in a short review of a large book, discuss this point more fully; but would merely further adduce the remarkable power the application of atropine has in checking recent cases of strabismus as very corroborative of the views we have been advocating.

The chapter on Operations for Artificial Pupil is not so clear in its arrangement as most parts of the book. Had the different operations been successively described, and under each the class of cases mentioned for which it might be employed, we think there would have been less confusion and obscurity. The operative procedures recommended in the different cases discussed we quite approve of, and merely take exception to the arrangement of the chapter.

The whole subject of Cataract, and the operations recommended for its removal, are treated of in a masterly manner. A strong predilection is manifested in favour of the old flap extraction compared with more recent methods of operating. We ourselves are rather in favour of a modified linear operation, but are quite willing to admit the neatness and applicability in very many cases of the older plan of extraction, which is ably advocated by Mr Walton.

In conclusion, we may remark that the woodcut illustrations, that are pretty plentifully interspersed throughout the text, are of more than ordinary merit, as are also the chromo-lithographs illustrating ophthalmoscopic appearances. A series of test-types are also appended, similar to Jaeger's, which we think might have been more valuable had they been constructed on Snellen's more scientific system.

We further feel compelled to take exception to some commendatory references to certain firms of opticians and surgical-instrument makers, which we think might be advantageously omitted.

While we have thus ventured to criticise what we consider capable of improvement in this book, we cannot but acknowledge the general excellence of the work. It is not a mere compilation or aggregate of other men's work, but contains much valuable original matter, and decided expressions of opinion on various subjects, on which, from his great experience, Mr Walton is entitled to speak with authority. On the whole, we can cordially recommend this as a safe and valuable book of reference.

Lectures on Skin Diseases. By E. D. MAPOTHER, M.D. Second Edition. Dublin: Fannin and Co.: 1875.

THE most ardent cultivator of that once neglected field—Cutaneous Medicine—cannot now complain of want of books on the subject. Time after time we are called upon to notice treatises on skin diseases, and in the recent works of Anderson, Wilson, Fox, etc., we now possess a literature in this department of which our country may well be proud. The sister country keeps pace with England and Scotland, and the name she has already won for herself in the study of cutaneous affections will, we think, be enhanced by the publication of Dr Mapother's little work, a second edition of which, in neat binding and good type, is now before us. Of course, like all authors, the doctor has got his own system of classification, which, while it has something to be said in favour of its simplicity, at the same time is not free from weakness and defect. We like his arrangement of acute and chronic effusive diseases, the former including erysipelas, erythema, urticaria, herpes, impetigo, pemphigus, chilblains, etc.; the latter, eczema, acne, and diseases of the sebaceous and sweat glands. The chapter, too, on the scaly affections and diseases of the cuticle and its appendages is very good, and so is the one on specific diseases. But into the portion of the work which treats of the diseases of the capillaries, Dr Mapother has introduced a great deal of matter most valuable and interesting in itself, but in our opinion wholly out of place in a volume professing to be lectures on the diseases of the skin. Nearly all he has written on purpura, for example, but more especially the able paper he contributes on cerebro-spinal meningitis, or, as he calls it, neuro-purpuric fever, might have been dispensed with, as not really and properly belonging to the subject in hand. The concluding chapter on baths, watering places, etc., is most nteresting and practical; and upon the whole we commend the little work to the notice of our readers who are interested in cutaneous affections, as a really valuable addition to the literature of that most common, most troublesome, and often puzzling class of diseases.

Della Cura Lattea. Par C. Dott Vivante, Medico-Chirurgo. Venice: 1874.

This is a pamphlet of 80 pages upon milk and the uses to which t may be put in medicine. There are few authors, from Hippocrates down to the latest medical periodicals, who have assigned to milk any curative properties that are not quoted. Milk is a nutriive substance with which we are all familiar, and in reading this pamphlet one almost regrets that it should ever cease to be the food of man. We have a good account of the milk treatment for diabetes, taken principally from English periodicals, and also of koumis in the treatment of phthisis. We imagine that there are very few physicians, however experienced, who will not learn omething new of the uses of milk by reading this little treatise; and if the learned author seem somewhat too ready to admit the urative properties of nature's food, at least there is very little fear of his prescriptions ever doing harm.

The New York Medical Journal. February 1875. New York: D. Appleton and Co.

This is an unusually interesting number of this spirited journal, nd contains, besides the usual original communications, reports, clinical records, etc., an interesting paper on Cholera by Dr Willis Alston, in which the particulars of a case are given which occurred sporadically in an isolated district and proved fatal, and no source of contagion could be traced. The present number likewise contains a portrait of Professor Jeffries Wyman, who died in September 1874.

On the Treatment of Fistula and other Sinuses by means of the Elastic Ligature: Being a paper (with additional cases) read before the Medical Society of London, November 1874. By WILLIAM ALLINGHAM, F.R.C.S., Surgeon to St Mark's Hospital, etc. Pp. 47. London: J. and A. Churchill: 1875.

THIS is a very interesting pamphlet, in which Mr Allingham details his method of using the elastic ligature in fistula in ano. He uses a solid indiarubber cord, and has devised a neat probe with a hook near its end, which can be projected after it has been passed through a fistula, so as to seize and carry a loop of indiarubber, either through the fistula or sinus or under a tumour. For the method he claims the following advantages:—

1. The operation is commonly painless, and the subsequent suffering, if any, is usually very slight.
2. There is greater rapidity of cure.
3. The patient need not keep his bed, nor even his room, but may go into the air, driving or walking in moderation.
4. Its peculiar applicability to delicate patients, and those who have a phthisical tendency.
5. It is bloodless.
6. There is a minimum amount of suppuration.
7. The ligature is often very advantageous as a supplement to the knife.
8. There is usually no anæsthetic required. (P. 14.)

All this sounds very well, still the conclusion of the paper is moderate and sensible:—

"I do not consider the elastic ligature can ever supplant the knife in the treatment of fistulous sinuses. In complicated cases the knife must be depended upon mainly; but I am of opinion that the indiarubber is valuable in many cases as a substitute, and in others as an auxiliary, to the usually employed method of excision." P. 47.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIV.—MEETING VII.

Wednesday, 5th May 1875.—Dr P. H. WATSON, *Vice-President, in the Chair.*

EXHIBITION OF PATHOLOGICAL SPECIMENS.

I. *Dr Clouston* showed the following specimens of morbid appearances in the brains of insane persons:—(1.) THE MORBID APPEARANCES IN THE BRAIN of a person suffering from an interesting form of GENERAL PARALYSIS. It has of late been regarded that this may originate by the peripheral propagation of a morbid process up to the brain, *e.g.*, from tabes dorsalis of the spinal cord, or from a crushed nerve in the hand, the Wallerian atrophy spreading upwards. In the case shown, it had been propagated from the retina. The patient, about seven years ago, had suddenly gone into cold water after being much heated by running. The result seemed to have been at first apoplexy of the retina. Next day he saw everything white, and this ended in total blindness. For the next five years he had intense pain in the vertex of the head, but there were no motor nor other sensory symptoms. About two years previous to his death he married, and a year afterwards he was seized with unsteadiness of gait, paralysis in arms, loss of memory, and ultimately the other symptoms of general paralysis supervened.

On post-mortem examination, it was found that there were the usual pathological accompaniments of general paralysis, in the vertex. The pia mater was found adherent to the convolutions generally, and, what was uncommon, to the base of the brain. There was thickening and condensation of the tissues round the optic commissure, and the serum under the arachnoid of the base was bloody. The optic nerves were only one-third their usual size, and grayish in appearance. On section, no nervous tissue could be detected in them, but only a gray fibrous material. The degeneration could be traced from the optic commissure, through the optic tracts to the optic thalami; and the posterior of the corpora quadrigemina were found to be shrunken and grayish. Now, it may be urged that the general paralysis, occurring five years afterwards, was a mere accident. But the fact of the general disease at the base of the brain, and also the tracing of the degeneration from the optic nerves directly to the great ganglia in the centre of the brain, negative this supposition.

There was no doubt in his mind but that the latent disease had been stirred into action by over-sexual intercourse. He wished finally to say that he believed there was only one other recorded case of such propagation from the retina.

(2.) and (3.) Two specimens of BRAIN TUMOURS, in both of which the symptoms were characteristic, but they were of a very different nature. The first was from the brain of a girl, twenty-three years of age, affected with syphilis. The characteristic symptoms before death were intense headache and epileptiform convulsions. The whole course of the disease was rapid, lasting only three months.

Post-mortem examination showed the presence of syphilitic deposits in the brain. In the right frontal lobe, there was one about the size of a hazel-nut; while another in the left frontal lobe was about the size of a pea.

(3.) A slowly growing TUMOUR, in about the same position as the above-mentioned syphilitic deposits. There were few symptoms before death, except gradually advancing weakness of the voluntary muscles and hemiplegia. The tumour was about the size of an egg. The patient had lived three years. [A full account of these most interesting cases will appear next Number.]

(4.) and (5.) The BRAINS OF TWO EPILEPTIC PATIENTS. In both, there was thickening of the dura mater, and in one of them there was a layer of adventitious tissue outside the dura mater, in a large space with small bony spiculæ in it. The skull-cap was eroded by it. There was no distinct history of any injury received.

In the other case, the patient had received an injury over the ear. He lived for ten years, and after death an irregular projection of bone was found below the stroke. There was thickening of the dura mater, and adherence to the brain, and gray and white softening of the convolutions. One curious point was the occurrence of bony deposits in the pia mater at the vertex. Near all these were apoplectic deposits, showing that they caused a good deal of irritation.

(6.) Also, a bottle full of GRAVEL-STONES, eighty in number, weighing twenty-four ounces, which had been swallowed by a patient in whom the tendency to suicide was most intense, and which had been passed at stool by him in the course of three days without a single bad symptom.

II. *Dr A. G. Miller* showed the termination of ANTERIOR TIBIAL ARTERY, which he had obtained from a limb amputated by him for a railway crush. The vessel had been torn across, and was therefore occupied by a clot which extended as far up as the first collateral branch. From nature in this way stopping the hæmorrhage, there had been little blood lost. The patient was doing well.

III. *Dr P. H. Watson* then gave an ACCOUNT OF A MURDER, some of the circumstances connected with which were interesting om a medico-legal point of view.

The case was one of a woman, seen alive on a Saturday night, but found dead in bed on the Monday morning following, under very suspicious circumstances. As her husband had been the only person in the house with her all the time, he was arrested on a charge of murder. The following is the report of the medical practitioners who examined the body of the murdered woman, as far as regards the wound made:—

"We found an incised wound penetrating the chest, 1¾ inch in length, and about 3 inches in depth, 2 inches from the lower point and inner edge of the left shoulder-blade. On opening the chest, we found the left lung collapsed, and a wound, 1 inch in length and ½ inch in depth, on the posterior part of the upper lobe, and behind it two pounds of dark blood, and the third rib fractured opposite the position of the wounds."

Now it was evidently of importance to settle if the wound had been inflicted by the husband with the intention to commit murder. From its position and direction, however, Dr Watson was inclined to think it had not been inflicted intentionally, while the clean division of a rib seemed to imply the use of very considerable violence. To satisfy himself as to these points, he performed the following experiment, assisted by his resident house-surgeon, Mr J. J. Gunn, and his class-assistant, Mr Jack. A dead body was procured and held perpendicularly up. It was then allowed to fall back, and as it fell Dr Watson advanced his hands, holding a carving-knife in his right hand, with a movement to catch the falling body. No sensation was conveyed to Dr Watson that even penetration of the chest had occurred. But it was found that a wound was thus inflicted, which, on examination, was seen not only to have penetrated the chest, but to have completely cut through the body of a rib external to its angle. The following is the particular description of this wound, as given in the report by himself and Mr Joseph Bell, who was associated with him in this case:—

"*Measurements of External Wound.*—3⅜ inches in length in a direction oblique from above downwards and from without inwards. Its lower end was 4⅜ inches from lower angle of shoulder-blade. Its upper end was 1⅜ inch from nearest point of shoulder-blade. From the spine of the prominent vertebra of the neck to upper end of wound was 3⅜ inches. From nearest point of the spinal column to lower end, 1⅜ inch. Distance between spine and shoulder-blade, 4⅛ inches.

"*Examination of Wound.*—Finger passed into centre of wound reaches a rib (the fifth) completely severed by a clean cut. Soft part in interspace below also completely divided. The rib above (fourth) is notched, but not severed. The measurement of the internal wound of the chest is 1$\frac{1}{16}$ inch. The sixth rib is also notched at its upper edge. The depth of the wound to level of rib is 1½ inch. Wound in lung is superficial, and 1⅜ inch in length."

From this experiment, then, as well as from a consideration of the reasons already given, he was able to show that the wound causing the death of the woman had not necessarily been inflicted intentionally by her husband, but might have been purely accidental, and this had led the prosecutor for the Crown to withdraw the charge of murder, and only press that of culpable homicide. The knife with which the exp riment was performed, and the ribs of the subject wounded, were shown.

IV. *Dr Watson* showed—(1.) A GAS-BURNER he had extracted from a boy's hand. The patient had been struggling with his brother to get possession of a pistol, for which the burner was being used as shot. While doing so the pistol went off, and as he had a hold of the muzzle, the burner lodged in his hand.

(2.) A CALCULUS passed from the urethra. Some time ago the patient had consulted him with symptoms of renal calculus. It afterwards passed into the bladder, but when seen occupied the prostatic portion of the urethra. Dr Watson had been unable to seize it either with forceps or a lithotrite in this situation. After the lapse of a few days, however, it passed without operative interference.

V. *Dr Miller* then read a paper on TWO CASES OF DISEASE OF THE KNEE-JOINT, TREATED BY SAYRE'S SPLINT, and showed the two patients whose cases had been detailed.

Mr Joseph Bell thought that the Society was indebted to Dr Miller for his interesting cases, and for the trouble, both mental and bodily, which he must have expended on them. Most of the members were aware of the advantages of extension in joint diseases. He did not think that, even with the most powerful extension, there was any separation of the articular surfaces of joints, but that it counteracted all spasmodic muscular jerking, and in this way kept the subchondroid layer of bone free from all disturbance. And, further, Sayre's apparatus added to this air and exercise.

Dr Sinclair was glad to see Dr Miller's improved method of using Sayre's splint for the knee-joint. He himself had been trying Sayre's method for eighteen months with most unsatisfactory results. He did not believe that, as Mr Bell had said, the extension apparatus acted directly on the muscles. He himself thought that it acted by keeping the articular surfaces from actual contact, so that any reflex action which might bring about muscular contraction was avoided.

Dr Watson, after remarking on Sayre's hip-joint splint and its hygienic influence, went on to say that he had tried Sayre's apparatus to the knee-joint, both in hospital and private practice. It certainly helped in synovial degeneration, with, however, the disadvantage of not preventing flexion. He had indeed given it up, being aware of Dr Miller's ingenious plan. He had instead an anterior splint, extending from the fold of the groin to the ankle, which he believed was original. He at first had used such splints made of tin, accurately fitting to the shape of the limb; but he now used splints made of wire about as thick as a penholder. This had an aperture for the knee-cap; the outline of this aperture was so padded as to give uniform pressure around the patella. By this means he not only got the limb straightened, but obtained complete absorption of the swelling caused by the gelatinous degeneration of the synovial membrane, even when bent at a right angle. The pressure thus applied was efficient in causing absorption of any gelatiniform swelling, but not so unequal as to cause excoriation. From his having used this apparatus now for more than two years with the most satisfactory results, he had quite abandoned Sayre's splint for the knee. Dr Miller's modification was simple and very ingenious, and the Society were much indebted to him for this practical demonstration of his method of application.

Dr Miller in reply said, that his reason for bringing forward Sayre's *knee*-splint was, that he was aware that the hospital surgeons had tried it and given up using it. He also had been on the point of giving it up, on account of the difficulty in keeping on the splint for any length of time. But the modification in its application shown by him, occurred to him in time to prevent his giving up the splint in despair. He did not agree with Mr Bell that the good resulting from the employment of the splint was due to its action on the muscles; for, when put on by his (Dr M.'s) method, there was very little pressure on the muscles or effect on them in any way. He did not agree with Dr Watson's objection, that Sayre's splint did not straighten the limb. Dr M. thought that the limb was more useful when a little bent, as it enabled the patient to get the advantage of the spring of the foot better in walking. He agreed with Dr Watson as to the hygienic advantages of this splint. It enabled the patient to go about very much the same as if the diseased joint were in the upper instead of lower extremity. But the splint effected more. Elbow-joints did not recover of themselves, but knee-joints with the Sayre splint on got well. He had not alluded to Mr Sayre's hip-joint splint, because it was in favour and in use. He considered the principle the same in both. Both obtained their hold for extension from the skin. He repeated (what he had stated in his paper) that he did not attempt to explain how dragging on a limb could effect its extension. But whatever the principle or theory of action of the Sayre splints, the facts related in his paper remained as proof of he usefulness of these splints.

Mr Bell thought that he had been misunderstood by Drs Sinclair and Miller. He did not believe that there could be sepa-ation of joint surfaces at all, and disagreed with Rust in thinking hat limbs were actually lengthened by the extension produced by ffusion of fluid into inflamed joints.

Dr Matthews Duncan, although not a surgeon, believed Dr Miller's cases to be interesting and instructive. He, however, entirely disagreed with him in regard to his ideas on the strength of the skin. In some experiments he himself had been conducting on the strength of animal membranes, he had found that the skin was stronger than even bone. He further thought that such theoretical objections should not be raised, when experiments might easily be devised to solve them. He believed that Sayre's splint was an apparatus pulling at a strong membrane in a proper way, just as a piece of paper could be easily torn, but when pulled fairly it had great strength.

Dr Watson said, that if he understood Dr Miller aright, he supposed that the point was not, whether the force acted at the middle point of the skin stretched, but how such stretching of the skin above and below a joint which admittedly took place, could affect the subjacent structures?

VI. *Mr Gilruth* then commenced to read a paper on the PHYSIOLOGICAL EFFECTS OF INJURIES OF THE SPINAL CORD IN THE LOWER ANIMALS, but unfortunately the Society was left in total darkness by the failure of the gas supply, and the meeting adjourned.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXIV.—MEETING VII.

Wednesday, 24th March.—Dr J. MATTHEWS DUNCAN, *President, in the Chair.*

I. *Dr Charles Bell* read a paper referring to CASES OF FATAL HÆMORRHAGE AFTER CHILDBIRTH, which appears at page 1096 of this Journal.

II. *Dr Ritchie* read the following communication by Mr William Donovan, Carrignavar, Cork, who had requested him to lay it before the Society:—

INDIAN HEMP IN POST-PARTUM HÆMORRHAGE.

Dr Connel's paper on post-partum hæmorrhage has suggested to me the propriety of laying a few facts connected with the treatment of uterine hæmorrhage by Indian hemp before the Society. I would not do so, but for the strange fact that (as far as I am aware) there is no mention made in any work on either midwifery or therapeutics of the action of Indian hemp on hæmorrhage from the uterine cavity.

In cases where flooding set in after delivery, a full dose of the tincture of Indian hemp (♏xx.) has, in every instance, acted rapidly, checking the loss in a few minutes, even when ergot has failed. I have also found that it possesses the power of controlling and relieving metrorrhagia and profuse menstruation in a marked degree. What the rationale of its action is I do not know.

Dr Keiller mentioned that, in the first case referred to in Dr Bell's paper, a post-mortem examination had been held, and the uterus had been sent to Professor Turner, while the fœtus had come into his own possession. It presented a very curious malformation, as it wanted the lower jaw, and was in some other respects imperfect. The second case related by Dr Bell he remembered but imperfectly; but knew that the patient made a slow recovery, having suffered, as is not unusual in such cases, from an inflammatory attack. She had had a child since, and all had gone well. He quite approved of the principles of managing those cases propounded by Dr Bell. Three nights ago he had seen a lady, a hæmorrhagic subject, in labour with her fourth child, who gave him much anxiety. From his former experience of this patient, he went prepared with ergot, and gave a dose as soon as the child was born. The uterus kept contracting and again expanding, and with great difficulty he succeeded in stopping the hæmorrhage by pressure outside with one hand, and inside upon the cervix and lower segment of the uterus with the other. He could not see how the perchloride could have been of any use in this case, as its direct application to the bleeding surfaces would have been prevented by the clots from reaching the uterine wall. The patient made a good recovery. He thought Dr Bell was scarcely justified in saying that the patient must inevitably have died had the perchloride been injected.

Dr Connel was surprised to hear Dr Keiller's last remarks, because Dr Barnes expressly says that it is necessary to clear out the clots before injecting the perchloride against the uterine wall. This he had done in his case, where, however, there were no clots of any size. The blood was all fluid. It was quite obvious that the clots should be taken away first.

Dr Keiller replied that all know that clots will form, and that it is very difficult to get them all away from the uterine wall. Many young practitioners, moreover, were afraid of removing the clots. He himself had been so when young; and, under such circumstances, he might possibly have used the injection. He believed that, in some cases, the perchloride had never reached the bleeding surface at all, and yet had got the credit of saving the patient. The very case he had just referred to, was one which, in the hands of Dr Barnes, might have been cited as illustrative of the life-saving property of the "marvellous styptic." Had the perchloride of iron (or even ergotine) been injected in this case, our journals or Society records would, in all probability, have proclaimed the old story of the *post hoc*, etc. Notwithstanding what his friend Dr Connel had said and urged in behalf of Dr Barnes's rules as to the use of the perchloride, he (Dr Keiller) from experience, affirmed that the mere squirting of any quantity of any so-called styptic against issuing blood and clots, such as we almost invariably meet with in post-partum flooding cases, is by no means required or even "equal to the occasion," and cannot be fairly entertained as an *essential alternative;* this opinion, however, did not affect Dr Keiller's high estimate of the perchloride in many other cases.

Dr Bruce was surprised at the number of cases in which it was reported to have saved life. He had seen many cases of severe post-partum hæmorrhage, but never a fatal one, and he had never used the perchloride.

Dr James Young remarked that it was the habit of some practitioners to compress the uterus *till* the placenta was expelled, then remove the hand to look after the child, allowing some considerable elapse of time before the binder or pressure was again applied; meanwhile clots of blood collect, and prove often the cause of severe post-partum hæmorrhage. His practice was to keep one hand on the abdomen almost all through the third stage, being assisted by the nurse, if necessary, in the removal of the child; and by keeping up firm steady pressure over the uterus both before and after the expulsion of the placenta, he prevented relaxation of the uterus, and consequent accumulation of clots and hæmorrhage. The binder does not and cannot prevent hæmorrhage, but he has no objection to its use when the perfect contraction of the uterus has been first secured.

Dr Duncan said that an important point had been raised as to the presence of clots when the perchloride is used. Now, if you waited until all the clots were away you simply waited till the patient was dead, while, if there were no clots, there could be no hæmorrhage; he accordingly failed to see where the perchloride was to come in. Again, to inject it while the blood is flowing would be equally useless, for he did not believe that any quantity of perchloride would stop blood which was still flowing—at least, not by its action as a styptic; those who had tried it on blood flowing from a large sinus or sinuses knew how inefficient it was. If of any use at all, it must be on other grounds. With regard to Dr Bell's first case, without wishing to criticise the practice followed, he thought it better under all circumstances to remove the whole placenta. With regard to Dr Donovan's paper, he would refer him to the records of this Society, where are to be found a number of experiments made with Cannabis Indica by Dr D. Christison, at the Royal Maternity Hospital. It was found of some use. Mr Donovan was quite in error in supposing that its use was not well known, for it was commented on in, he would venture to say, almost every recent work on gynæcology as a remedy which had been tried in various forms of menorrhagia. Dr Churchill's book on Diseases of Women perhaps contained the fullest account of it.

Dr Bell, in reply to Dr Duncan, said that it was impossible to remove the small piece of placenta which was left behind, without serious risk to the patient, as it was firmly adherent; all that was movable he had removed. With regard to death being the itable result of injecting the perchloride, he considered that in consequence of the uterus relaxing and contracting as it did in the case referred to, it would absorb the solution like a sponge, producing fatal embolism. He complained that we are left in the dark, by all English authors who are favourable to the practice, as to what is the real effect of the injection upon the uterus. Does it really cause the uterus to contract? That was the all-important question. If not, it was worse than useless. He thought it might be useful in some cases of laceration of cervix and similar causes of hæmorrhage, as is stated in the paper; but it was too much diluted by Barnes to produce any benefit, even in those cases.

III. *Dr Underhill* read a paper on a CASE OF INTRAUTERINE POLYPUS, which appears at page 1098 of this number of the Journal.

After a few remarks by *Dr Keiller*,

Dr D. Murray wished to know what ought to be done with polypi which come down during labour and obstruct the passage of the fœtus. He had had such a case, and had lost the child in consequence.

Dr Duncan observed that there is more than one kind of descending polypus. He had lately removed one from the cervix, which came down occasionally, in an old woman; some with very long pedicles hang permanently outside, in others the protrusion is only occasional; but cases which descend and then go back within the uterine cavity are not very uncommon. Dr Underhill's case was not a perfect example of this condition. He had under his care one case in which a married lady suffered from very severe hæmorrhages at each menstrual period. She was sent to him as a case of fibrous polypus, but there was no polypus to be made out when he first saw her, and the os was closed. At the next menstrual period, however, a tumour of considerable size came down, and the usual hæmorrhage took place. The tumour was removed, and the patient recovered. He had seen other such cases. The polypus is expelled by uterine contractions, but he could not conceive how any form of uterine contraction could pull it back again. This was done entirely by the positive or inspiratory action of the retentive power of the abdomen, in the same manner that air and foreign bodies are at times drawn into the vagina and bladder.

Dr Keiller said the os would contract after expelling the body, and would expand again when the polypus was pressed up from any cause against it from below. Such may have happened in this case.

Dr Underhill, in reply, said that his object in reading the case was to obtain an explanation of the process of retraction, and was much obliged to Dr Keiller and the President for their remarks.

IV. Dr Vassily Sutugin, of St Petersburg, was unanimously elected a Corresponding Member of the Society.

THE UNFORTUNATE MRS MARSDEN.

THIS poor uneducated midwife now lies in prison undergoing punishment for a new and hitherto unheard-of crime, homicide by puerperal infection. All that has been proved against her—that is, having in succession two or three puerperal deaths—may any day be proved against some practitioner or other; for we have no doubt that, as in times past such events have occurred in the practice of eminent men in the profession not unfrequently, so they are happening now. Indeed, one needs only to keep an eye on the weekly medical journals to see frequently reports of such events. It is under such circumstances that Mrs Marsden has been accused, tried, and condemned to a grievous punishment. We believe this proceeding by the authorities to be most injudicious and unjust, and we hope that means will soon be taken to get the prison doors thrown open, that she may go free.

The legal difficulties in the way of any possible just condemnation under the circumstances are so great, that we cannot sufficiently wonder at and deplore the course of so-called justice in this case. Some of these difficulties we shall put as questions in the mouth of a defender such as Mrs Marsden ought to have had.

Is there such a disease as puerperal fever?

How was it ascertained that the cases under consideration were cases of puerperal fever?

Was post-mortem investigation resorted to?

If so, were the results found sufficient to distinguish the disease from the diseases liable to be confused with puerperal fever?

Is puerperal fever infectious?

Can you tell when the disease is got by infection and when it occurs without infection?

Were the cases under consideration the result of infection by Mrs Marsden?

Did Mrs Marsden neglect proper care?

To all of these questions some answer, generally an affirmative answer, should have been justly given before a conviction could be justly got. But, so far as we know, these considerations were either utterly neglected, or an answer in favour of Mrs Marsden can be given to them. Had they been weighed with due insight by the authorities, the case could not have ended otherwise than in the dismissal of Mrs Marsden from the bar.

We grieve to observe that medical men were found so ignorant or inconsiderate as to give unqualified support to the charge against the poor midwife. It appears to us quite evident that some, if not all, of these should have, on their own evidence, been asked to descend from the witness-box and take their places beside the so-called criminal at the bar; and, indeed, their conduct was in some respects much worse than the criminal's, for they were educated n, and should have known better. We find such evidence as t this or that medical witness attended Mrs Marsden's fatal cases with her or for her, but we find no evidence that they gave up practice. Indeed, we think we have read that they not only attended her fatal cases, but offered to take her other practice off her hands. They were giving evidence against Mrs Marsden, whose crime consisted essentially in not discontinuing practice, yet they themselves continued practice. They did the very thing for doing which they succeeded in getting Mrs Marsden punished. The crime in both cases was the same, though the results to the patients may have been different.

Analogous pieces of illogical absurdity may be, perhaps innocently, played off upon patients, but cannot surely be passed over in a court of law in such a grave case as Mrs Marsden's. We have known an obstetrician asked by a patient to give up her case because he had a case of puerperal fever in his hands, and of course the obstetrician consented to do so for evident sentimental reasons, if for no other. This patient employed another obstetrician who happened to have two such cases in his hands—a circumstance of which the patient was ignorant, and the practitioner was wise enough not to tell her. This is a kind of absurdity of conduct that cannot and should not be stopped in patients. But we cannot stand by and see similar absurd conduct in medical brethren, who are using their influence to get an humble practitioner punished for doing what every medical man does. There is no kind of medical man who does not constantly expose his patients to some risk. In Mrs Marsden's case nothing more was proved than that she exposed hers to some risk.

We hope that an appeal will be made to the Home Secretary to have this unfortunate and ill-used midwife released from prison.

Part Fourth.

PERISCOPE.

Dr Prokop Rokitansky contributes a paper *On the Influence of Chloral Hydrate on the Excitability of the Nervous System* to *Stricker's Jahrbücher* (III. u. IV. Heft) for 1874. He concludes, from the results of experiments on rabbits, that—

I. Chloral hydrate is a direct poison to the heart. When introduced into the heart in certain doses by a Wood's (Pravaz) syringe through puncture, either of the jugular vein or the walls of the heart itself, the animal dies by stoppage of the heart's action.

II. It diminishes the excitability of the motor centres of the respiratory muscles, and of the muscles of the trunk.

III. It diminishes the excitability of the vaso-motor centres.

Schlesinger. *The Temperature of the Uterus.*—The result of a number of observations presented by the author in a tabular form, shows that the vagina is on an average 0·23 (Centigrade) warmer than the axilla, and the cavity of the normal unimpregnated uterus 0·19 warmer than the vagina. The difference between the rectum and vagina was unequal; but on an average the vagina was warmer. The difference between the canal of the cervix and the cavity of the uterus was, on an average, 0·1 in favour of the latter. —*Stricker's Jahrb.* 1874.

SCHRÖTTER, in an address to the Vienna *Gesellschaft der Aerzte* in June 1874, gave his further experiences in the treatment of stenosis of the larynx. [The method of treatment consists in the insertion of bougies through the glottis, which are fixed above by a ligature which passes through the mouth, and by a special forceps, which seize the lower end, through a canula introduced between cricoid and thyroid cartilages.] He had up to that time treated eight cases, and by these it had been shown, that (1) even in the most difficult conditions, the lumen of the larynx can be artificially restored; (2) that the procedure is perfectly harmless to the patient; (3) that the sensibility of the larynx is not impaired; (4) that the duration of the treatment must in any case be prolonged, and always must be reckoned by months.

The Aleppo Evil or Button (*Bouton d'Alep*).—For more than a century physicians and travellers in the East have described a special form of skin disease as being "endemic about the Tigris and Euphrates, at Aleppo, Bagdad, and Bussorah" (T. Fox). Dr Geber of Vienna has lately had the opportunity, during his travels in Syria and the Levant, of examining with care the nature of this supposed special malady. In Aleppo, during a somewhat prolonged stay, he opened a dispensary for skin diseases, in order to have material for observation; and he was further assisted in his studies by the resident physicians. He has published an extensive and detailed account of his observations, and the grounds on which he rests his conclusions, in the *Archiv f. Dermatologie u. Syphilis* (1874, 4 Heft). He found that the cases of so-called Aleppo evil were either syphilis, lupus, scrofulous ulcers, or, in rarer instances, dermatitis, provoked and sustained by filth. In 181 cases, 57 were syphilis. Syphilis and lupus are frightfully common amongst the population of the whole adjoining district. In a Maronite school, amongst 130 children, 24 were found to be suffering from lupus vulgaris, and 10 from syphilis ulcerosa. He gives the following definition of the disease:—

"In the great majority of cases, in the whole of these Eastern countries syphilis, lupus, scrofulosis pass amongst physicians and travellers as the boil of Aleppo, Bagdad, Diarbekir, etc."

Dr Geber is a distinguished teacher of dermatology in the Vienna school, and from his great experience as Hebra's assistant, is testimony carries with it much weight.

Krügkula. *Four Cases of Infection from Vaccine Lymph, and Fatal Termination* (*Wien. Med. Wochensch.* 1874, No. 47).—Eight soldiers were vaccinated with lymph which was procured from a foundling hospital. In two of them inoculation did not follow. The other six, in whom the vaccine pustule quickly developed, took ill after twenty-four hours, with rigors, violent fever, great prostration, and delirium. In two to four days after the inoculation, a phlegmonous inflammation manifested itself on the arm, in some on one side, in others on both sides, which, after several days, ended in gangrene. Two of the patients recovered, four died. From the same source, other nine glasses of lymph were taken, and their use was followed by no morbid symptoms.—*From the Centralblatt*, No. 13, 1875.

Haab. *Experimental Investigations into the Growth of Long Bones in Pathological Conditions.*—It is known that irritation of the shaft of a long bone, when not too near the epiphysis, produces abnormal growth in the length of the bone. Haab has found, in experiments on young rabbits, dogs, pigeons, hens, and cats, that the irritation produced by the application of pins, rings, and flat bodies under the periosteum is not limited to the bone to which the application is made. When the tibia was pierced or grasped by a tight ring, that bone was not only abnormally lengthened or shortened, but the femur of the same side, which had been left intact, became abnormally longer or shorter. The lengthening of the femur may even exceed that of the tibia. These experiments confirm an hypothesis of Langenbeck's, who, in 1869, suggested that, in cases of morbid excessive growth of the tibia, the fibula and femur may also lengthen abnormally.—*Centralblatt*, No. 13, 1875.

P. Foet. *Attack of Hysteria in a Man, Treated and Cured by Compression of the Testicles* (*Gazette Hebdom.*, 1874, No. 50.)—A man, who was lying unconscious and violently convulsed, the whole surface of the body showing itself insensible to impressions, was restored to consciousness by firm compression of both testicles. The patient was liable to paroxysms of pain in the right temple, and had been afflicted with melancholy the day before the attack. —*From the Centralblatt*, No. 15, 1875.

Haberkorn (*Cure for Gonorrhœa, Berlin. Klin. Wochensch.*, 1874, No. 34) recommends, both in recent and older cases of gonorrhœa, small injections, thrice daily, of quinine 1 part, water 75, and glycerine 25, with a few drops of acid to produce solution.—*From the Centralblatt*, No. 2, 1875.

Wound of the Liver.—Dr G. W. Stewart reports the case of a farmer's boy, æt. 17, who was knocked down by a runaway team in front of his plough, the point of which struck him in the back. It inflicted a wound eleven inches in length, extending from the fourth to the first lumbar vertebra, in the left hypochondriac region, engaging the twelfth rib and denuding it of its periosteum in the anterior third, passing through the peritoneum, entering the liver at the left lateral ligament, and proceeding in the direction of the intersection of the longitudinal with the transverse fossa, within which, and only a few lines beyond the terminus of the incision, could be distinctly felt the hepatic artery. Nearly or quite all of that portion of the left lobe of the liver anterior to the incision was found protruding through the wound. The case was seen about two hours after the accident; hæmorrhage had ceased, but the patient was nearly comatose from want of blood, a large quantity of which was removed from the abdominal cavity with a sponge; the stomach was empty and had escaped injury, as had also the intestines, diaphragm, and other organs. The abdominal aorta could be distinctly felt in the floor of the wound.

The protruding portion of the liver was replaced with some difficulty, and retained by a compress and a wide roller bandage; cold water was applied to the wound, which was closed up; the patient was placed on a bland and restricted diet, and small doses of opiates were administered. The compress was removed at the end of twenty-four hours, but no other change was made for some time. The wound began to discharge pus freely about the sixth day, the patient gained strength from the first, and no indications of peritonitis appeared at any period. About the twelfth day the flaps sloughed off, and exposed to view the liver and also the twelfth rib. A portion of the former, equal in size to a two-inch cube, was again protruding through the wound, and was irreducible, owing to adhesions between the borders of the left lobe and the abdominal wall. Caustic was applied daily, and the cauterized material afterwards removed with a knife, until the prominence had disappeared. About one half of the exposed and projecting rib was then removed with bone-forceps. The wound then improved in condition, the two fragments of liver united perfectly, the cauterized surface healed speedily, granulations sprang up rapidly in the muscular tissue, and continued until the divided muscles were reunited and integument formed over the entire surface. The case terminated in complete recovery in about seventy days.—*Chicago Medical Journal*, September 1873.

METHOD OF INCREASING THE POWER OF THE OPERATOR IN CASES OF DELIVERY BY FORCEPS.—Under this heading, in the *Repertorio Jalisciense de Medicina y Cirugia* for 15th January, Dr Fuentes writes:—"One of the difficulties experienced in making use of the forceps is owing to the powerful efforts which are sometimes required to overcome the resistance met with in extracting the fœtus. These efforts frequently exceed the power of the obstetrician, who, for want of a skilled assistant to help him, may find himself obliged to suspend the operation, confess his want of physical power, or to avail himself of unskilled assistance, which involves a thousand inconveniences, and may even prove a source of danger.

"In circumstances such as these I found myself at the side of a woman in labour, who, from the relative disproportion in the diameters of the pelvis as well as the gradual weakening of the expulsive powers, required the application of the forceps. It was night, an hour very inconvenient for soliciting the aid of a friend. After fruitless attempts at traction my strength failed; a very copious sweat bathed my body, and my exhaustion was inexplicable. A practical idea occurred to me: I called to mind the manner in which brass-workers lengthen wire by drawing it through a wire-plate. I immediately asked a strong girdle, which I fastened round my waist, and attached this band to the free extremities of the forceps conveniently applied. Then resting my knee on the edge of the bed and pulling *methodically*, simultaneously with my waist and arms, I was able without much difficulty to extract the fœtus alive and unhurt.

"I do not know whether this method of facilitating the management of the instrument may have occurred to others, but I am persuaded that, should any one of my professional brethren find himself in similar difficulties, he will not disdain this advice, but will avail himself of a resource which he cannot reject as unsuitable."

Part Fifth.

MEDICAL NEWS.

MIDWIVES AND PUERPERAL FEVER.

Communicated by THOMAS N. ORCHARD, M.D.

AT the Manchester Assizes, on Saturday 18th March 1875, before Mr Justice Field and a special jury, Elizabeth Marsden was indicted, that "on the 16th of February last, in the borough of Salford, she did feloniously kill and slay one Mary Ellen Goodier;" and that "on the 19th of February last, in the borough of Salford, she did feloniously kill and slay one Ann Mills." To both indictments the prisoner pleaded "Not guilty." On the 17th of January last, Mrs Marsden attended in confinement a woman named Carroll, who, a few days after, was seized with puerperal fever and died. About the same time, or shortly after, she attended two other labour cases. Both patients suffered from the fever; one of them died, and the other, contrary to expectation, recovered. The houses in which those cases occurred were situated at a considerable distance from each other, and there was no intercourse or communication held between any of the persons who resided in them. After the occurrence of the third case, I went to Mrs Marsden and told her that I had visited three patients suffering from puerperal fever who had been recently attended by her during their confinements, and that two of them had died. I explained to her that the disease was very contagious, and that there could be no doubt but that she had communicated it to them. I also advised her to abstain from attending any more cases for two or three months. Two other medical men cautioned her in a somewhat similar manner. Without paying any attention to the warning given her, Mrs Marsden attended two other cases, both of which died of puerperal fever, one on the 16th, and the other on the 19th of February. A warrant having been issued against her, she was apprehended and committed for trial at the next Assizes, on the charge of manslaughter, bail having been refused. The case was gone into very minutely, and lasted over five hours. The Judge, in summing up, asked the jury to set aside all personal feeling that might have been aroused. It was utterly immaterial whether the prisoner was a certificated midwife or not, or whether she took a large reward for her services or a small one; because the law was, that any person who undertook the duty of delivering a woman, was bound to bring due care and skill in the performance of that duty. And having undertaken the duty of attending a woman in her confinement, and knowing, as a midwife must know in her experience, the danger of communicating contagion, and knowing the circumstances of the case, the question was, Whether she had been guilty of a gross and criminal negligence in performing that duty? The jury, after a few minutes' deliberation, found her guilty. The Judge, in passing sentence, addressed the prisoner and said, "This is a matter which the law cannot pass over, because death was caused by negligence of this kind, after what, I am bound to say, was a very plain warning, which you ought to have attended to. Under the circumstances, I shall pass as light a sentence as I dare do, which is, imprisonment for six months."

This case is interesting as being the first on record in which the prosecution of a midwife for communicating puerperal fever was followed by conviction. It also establishes an important precedent with regard to the occurrence of any more of such cases in future, either among medical men or midwives; and that the law will not permit any gross negligence or want of due care and skill to pass unnoticed. The counsel for the defence endeavoured to mitigate her responsibility by pleading "ignorance," and arguing that she received no course of instruction after the manner that certified midwives do. Perhaps, upon this point, she deserves some commiseration. The necessity for some legislative interference to compel a woman to qualify herself by going through a course of instruction before practising as a midwife, is daily becoming more apparent; and it is to be hoped that by-and-by no woman will be allowed to call herself a midwife, unless she has a certificate or diploma authorizing her to practise as such.

PENDLETON, MANCHESTER, 19*th April* 1875.

Bills.—We have received several communications in regard to professional bills. One correspondent from Boston sends us a blank form which he uses, and which he says is very commonly employed in that city. The front of the bill presents nothing unusual, but on the back of it is printed as follows:—

The Boston Medical Association, composed of the regular physicians of Boston, adopted the following Fee-Table, 1st July 1864. But it is left to the judgment of each practitioner to make any deductions from the following rates which the pecuniary circumstances of the patient may require.

For each visit, within the city, in the daytime . . .	$3·00
For a visit after 9 P.M. and before 8 A.M.	$5·00 to $10·00
(In cases of consultation or other extraordinary attendance in the night, the fee for such extra attendance shall be added to that for a night visit.)	
For a visit in consultation	$5·00 to $10·00
For attendance involving travel out of town, mileage shall be charged at a rate per mile, for short distances, of .	$1·00 to $2·00
For advice at the physician's house, according to the importance of the case, unseasonableness of the hour, or time occupied	$3·00 to $20·00
For Vaccine Inoculation	$5·00
For attendance in Midwifery in the daytime . . .	$20·00
For " " in the night	$30·00
(Obstetric Operations, when necessary, shall be charged in addition to the usual fee for attendance. In obstetrical practice all subsequent visits shall be charged as in ordinary cases of attendance.)	
For Minor Surgical Operations, such as stitching wounds, opening abscesses, etc.	$5·00 to $25·00
For Major Operations, according to importance . .	$25.00 to $500·00
(After surgical operations, all subsequent visits shall be charged as in ordinary cases of attendance.)	

—*Philadelphia Medical Times*, 16th January 1875.

OBITUARY.

Since our last issue, the profession in Edinburgh has lost three of its members, all at a comparatively early age, and under circumstances of sadly sudden nature,—

Dr Daniel R. Black, whose singularly retiring and modest disposition did not prevent those who knew him from recognising great ability and conscientious discharge of duty;

Dr William Hammond, who, after a successful period of country practice, had begun to be well known in Edinburgh. He died of fever, caught in the laborious discharge of his parochial duties; and, lastly,

Dr Cairns, well known as an able private teacher of medicine, and a frequent writer in medical periodicals.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

Edinburgh, *6th May* 1875.

Sir,—I beg to enclose the annexed short note on a method of improving the patient's appearance in the last stage of ophthalmia tarsi.

It has never before been suggested or adopted, so far as I know, and it is worthy of trial.—Sincerely yours,

SOMERVILLE OLIVER, M.D.

In the advanced stage of ophthalmia tarsi—the stage sometimes termed "lippitudo"—when the eyelashes have disappeared, and the edges of the eyelids have become rounded, perhaps tumid and everted, after all has been done for the patient that can be done by the usual means adopted in such cases, the unseemly appearance caused by the loss of the eyelashes and the red margin of the lids is still very conspicuous; but it may, I think, be considerably lessened by *tattooing* the *margin* of the *eyelids* as darkly as possible with Indian ink.

By using Snellen's eyelid forceps, there can be no risk of pricking the eye itself. Darkening the margin of the lids by tattooing would also mitigate the disfigurement caused by removal of the cilia and their bulbs in cases of trichiasis and distichiasis.

PERIODICALS RECEIVED.

American Journal of Insanity,—Jan., April.
American Journal of the Medical Sciences, —January.
American Journal of Obstetrics,—Feb, May.
American Practitioner,—Dec. to May.
Annales d'Oculistique,—Nov. to April.
Annali Universali di Medicina e Chirurgia, —January, February, April.
Archives Générales de Médecine,—Jan. to May.
Archives de Physiologie,—Nov. to April.
Archives de Tocologie,—Jan. to May.
Archives of Dermatology,—January, April.
Archives of Electrology and Neurology,—November.
Australasian Medical and Surgical Review, —October to March.
Berliner Klin. Wochenschrift,—December 28 to May 17.
Births, Deaths, and Marriages, Monthly Return of,—December to April; Quarterly Return,—December.
Boston Medical and Surgical Journal,—January 7 to 21.
British and Foreign Medico-Chirurgical Review,—January, April.
British Medical Journal,—December 26 to May 22.
Bulletin Général de Thérapeutique,—Dec. 30 to May 15.
Bulletin et Memoires de la Société de Chirurgie de Paris,—No. 1.
Chicago Journal,—October, January, April.
Detroit Review of Medicine,—Jan. to April.
Druggist,—November to April.
Dublin Journal of Medical Science,—Jan. to March.
Echo de la Presse Médicale,—January 8 to May 8.
France Médicale,—December 23.
zette des Hôpitaux,—Dec. 24 to May 20.
tte Hebdomadaire de Médecine,—Dec. o May 21.
Gazette Médicale de Paris,—December 26 to May 15.
Glasgow Med. Journal,—Oct., Jan., April.
Guadalajara Repertorio Jalisciense de Medicina y Cirugia Practicas,—Nos. 3 to 9.
Indian Medical Gazette,—Dec. to Feb., April.
Journal de Thérapeutique,—December 25 to May 10.
Journal of Anatomy and Physiology,—Nov.
Journal of Mental Science,—Jan., April.
London Medical Record,—Dec. 30 to May 19.
Medical Press and Circular,—December 30 to May 19.
Med. Times and Gaz.,—Dec. 26 to May 22.
Medizinische Jahrbücher,—1874, Parts 2, 3, 4; 1875, Part 1.
New York Medical Journal,—Jan. to May.
Nordiskt Medicinskt Arkiv,—Sjette Bandet, Fjärde Häftet.
Obstetrical Journal,—January to May.
Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool,—Derde Reeks, Aflevering 1.
Philadelphia Medical and Surgical Reporter, —December 19 to May 8.
Philadelphia Medical Times,—December 12 to May 8.
Physiological and Medico-Legal Journal,—December.
Practitioner,—Jan. to May.
Progrès Médical,—Dec. 26 to May 15.
Revista Medica de Chile,—Nov., Dec., Feb.
Revue Scientifique,—February 13.
Revue de Thérapeutique Médico-Chirurgicale,—January 1 to May 15.
Richmond and Louisville Medical Journal, —January.
Sanitarian,—Jan., Feb., April, May.
Sanitary Record,—Jan. 2 to May 22.
Students' Journal,—January 30.
Vierteljahresschrift für Dermatologie und Syphilis,—1874, 2 und 3 Heft.
Virchow's Archiv,—Dec. 5 to April 9.

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3d March 1875.—Exhibition of patients by Mr Joseph Bell; exhibition of specimens by Drs Watson, Keith, Robertson, Miller, Dunsmure, and Mr Bell; paper on grouse disease by Mr A. Wilson; paper on conservative surgery by Dr Macdougall of Galashiels, with remarks by Professor Spence, Dr Watson, and Messrs Chiene and Bell, 932–939.

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13th January 1875.—Specimen by Dr Cairns; case by Dr James Young; adjourned discussion on Dr Counel's paper on post-partum hæmorrhage, by Drs Pattison, Coghill, Craig, Carmichael, Young, Matthews Duncan, Keiller, and Connel; Dr Sutugin's paper on length of gestation, 844–852.

27th January 1875.—Exhibition of plaster cast; case of fœtal monstrosity, by Dr Macdougall, with remarks by Dr Keiller; cases of hair in female bladder and fibroid polypus, by Dr Macdougall, with remarks by Drs Simpson, Young, and Keiller; case of central rupture of the perineum, by Dr Wilson of Alloa, with remarks by Drs Burn, Hammond, Keiller, Simpson, Young, and Gordon, 939–945.

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END OF TWENTIETH VOLUME.

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